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THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

SIR WILLIAM THOMSON, KNT. LL.D. F.R.S. &c.

AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.

J. B. Pinelli ad Mazonium.

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ERRATA.

Page 198, line 17, *for* 12½ per cent. *read* 12½ cent.

— 284, lines 9 and 13 from top, for $\frac{e_1}{q}$ read $\frac{q}{e_1}$.

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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JANUARY 1881.

I. *On the Absorption of dark Heat-rays by Gases and Vapours.* By ERNST LECHER *and* JOSEPH PERNTER*.

[Plate I.]

MAGNUS and Tyndall, who, as is well known, have investigated (the latter through a long series of years) the absorption of radiant heat by gases and vapours, arrived, on some points, at very different results. Other investigators also have devoted time and trouble to this investigation, and in particular to the absorption of heat-rays by aqueous vapour, without being able to arrive at any final decision.

The difficulty of the observations in question, perhaps on few other questions of experimental physics so great as on this, hardly permits the employment of a method entirely free from objection; hence the explanation of the fact that hardly ever do we meet with such differences, and even contradictions, as here. Thus, for example, Tyndall † finds that a layer of pure dry air of 1·22 metre thickness transmits all the rays of heat given off by a source of heat at 100° C.‡ Magnus §, on

* Translated from a separate impression, communicated by the Author, from the *Sitzb. der k. Akad. der Wissensch. in Wien*, July 1880.

† Tyndall, 'Contributions to Molecular Physics in the Domain of Radiant Heat.' (London, Longman, Green and Co., 1872.) We cite this work whenever possible for the investigations of Tyndall, since he has himself carefully collected all his different researches on the subject in this work.

‡ *Loc. cit.* p. 19. § Pogg. *Ann.* cxii.

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the contrary, finds that a layer of air 275 millims. in thickness is sufficient to absorb 11 per cent.; and, lastly, Buff* believes he has observed an absorption of 40 per cent. by a layer only 45 millims. in thickness. It is remarkable that these results are in inverse proportion to the length of the absorbing layer, and so illustrate only the more strikingly the inexplicable difference in the results of such excellent experimenters.

The uncertainty which these results left on the one hand, and on the other the indubitable absorption of heat-rays in the atmosphere, especially as shown by the pyrheliometric investigations of Violle† and Crova‡, induced us to undertake the present investigation. If it should lead to the conclusion that the question can hardly be decided by absolute measurements in the laboratory, yet the results obtained may at least furnish definite starting-points for its decision. Further, the numbers given by Tyndall for the vapours have been subjected to accurate investigation; so that some new points of view present themselves in respect to physics.

When, in what follows, we give numbers which for the most part deviate from those previously found, we are well aware of the difference, and wish at the outset to emphasize the fact that the values in question, in spite of their deceptive coincidence with each other, are to be regarded as only somewhat rough approximations. But we shall show immediately that all numbers hitherto given, including those of Tyndall which relate to vapours, are vitiated by important errors resulting from the mode of observation employed.

Remarks on Methods previously employed.

There are two essentially different methods which may be employed: the thermopile and the source of heat may be placed in the same chamber in which the gas or vapour under examination is placed; or one, or both, may be outside of the space filled with gas. The first method has been employed especially by Magnus §, and then by Garibaldi‖ and by Buff¶.

* Phil. Mag. [5] iv.; Pogg. *Ann.* clvii.

† *Comp. Rend.* t. lxxxii. 1868, 1.

‡ *Mesures de l'intensité des radiations solaires*, par M. A. Crova. Paris, 1876 (Gauthier-Villars).

§ Pogg. *Ann.* cxii.

‖ *Il nuovo Cimento*, ser. 2, t. iii. This work was accessible to us only by means of the reports in the *Naturforscher*, Jahrg. iv. 1871, Nr. 33, and in *Fortschritte der Physik*, 1871.

¶ Phil. Mag. [5] iv.

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Without entering into details, we may remark that Tyndall has shown that, with the source of heat in the experimental space, currents of air and conduction of heat are unavoidable; and this source of error is obvious. Moreover, so far as the literature of the subject is known to us, Magnus does not return to this question; so that apparently he has himself recognized this source of error.

Buff endeavoured to avoid this error by rapid heating of the surface which serves as source of heat; but the duration of the experiment is always much too great, and, especially, currents of air and conduction are not avoided. The most striking proof of this is found in Buff's tables, since the greater deflection always takes place after the heating has been interrupted for some time, which could not have been the case if the heat radiated by the heated surface had been the sole cause of the deflection, as is required by the conditions of the experiment.

Garibaldi moreover employed a concave mirror to concentrate the heat-rays; and we suspect that in this is to be found the reason of the incredible result which he obtained of an absorption of 92 per cent. by aqueous vapour.

Tyndall*, as is well known, first employed the second method. Since the space containing the gas is closed and yet must be open to heat-rays, the tube employed must be closed on both sides by diathermanous substances. Tyndall employed plates of rock-salt. Dr. Franz† had previously employed glass plates, which, in consequence of the great absorption which glass exerts upon dark heat-rays, must clearly lead to very fallacious results. Tyndall's method was also employed by Wild‡ in determining the absorption of aqueous vapour; but since the arrangement was precisely the same as Tyndall's, the results must be open to the same objection.

Since the perfect diathermancy of the plates which close the tube can never be attained, there must always remain a source of error in this arrangement. And even if the original objection of Buff's, that rock-salt and air are of similar colour for heat-rays, is not confirmed, it is at any rate clear from Tyndall's own researches § that rock-salt absorbs 16 per cent. of the radiation from a lampblack surface, and indeed, as he expressly maintains, in opposition to Melloni and Knoblauch, that rock-salt does not transmit all rays in equal proportion.

Buff has maintained that Tyndall found no absorption produced by air, simply because air and rock-salt absorb the

* *Loc. cit.* Memoir I.
† Pogg. *Ann.* xciv.
‡ *Ibid.* cxxix.
§ *Loc. cit.* p. 322.

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same rays in almost exactly equal proportions. The diathermancy of rock-salt has always been examined in air, and then the diathermancy of air between rock-salt plates. This objection is completely justified in the employment of Tyndall's method; and it is necessary that the diathermancy of rock-salt should be examined *in vacuo*. But our experiments made without rock-salt plates have shown that Tyndall's numbers, so far as they relate to air, are perfectly correct*.

The employment of rock-salt plates may therefore be allowed in experiments with dry gases, since the percentage of rays absorbed does not appear to be materially influenced by the imperfect diathermancy of the rock-salt.

The case, however, is altogether different with vapours. Tyndall's numerous replies to the objection made by Magnus, that on the walls of the polished tube and on the inner surfaces of the rock-salt plates vapour-adhesion must take place, are by no means convincing.

We wish from Magnus's deductions from particular experiments, according to Tyndall's own data, to collect a convincing proof of the magnitude of vapour-adhesion. Tyndall has remarked that it may not be a matter of indifference whether the interior of the tube be polished or not. He recognized that exact investigation alone can set at rest the doubt whether the results will remain in the same proportion amongst themselves if the tube be blackened instead of being polished. It would have been the safest to make similar experiments with similar tubes, in the one case polished and in the other blackened, and to take special care that all other conditions remained unaltered. But Tyndall blackened only the half of the tube, and, in the experiment with this last, employed only a vapour-pressure of 0·3 inch, whilst in the experiments with the polished tube 4 feet long the vapour-pressure was 0·5 inch.

With such an inequality in the conditions, it can be only a chance that the numbers given in Tyndall's table for the absorption of the vapours examined are in the same proportion for the blackened and for the polished tube. Tyndall finds that it is only necessary to multiply the numbers found for the blackened tube by 4·5 in order to obtain the numbers for the polished tube.

Moreover the numbers given do not agree so very well together. Table XVIII.† contains nine substances, for which the numbers are as follows:—

* Moreover Tyndall has recently directly refuted Buff's objection (Proc. Roy. Soc. vol. xxx. p. 19). This paper is known to us only by the report in *Naturforscher*, Jahrg. xiii. No. 16.

† *Loc. cit.* p. 35. This and all following translations are as literal as

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Vapour.	Absorption per 100*. Blackened tube. Pressure 0·3 inch.	Polished tube. Pressure 0·5 inch.	Absorption in blackened tube proportional to
Sulphide of carbon...	5	27†	23
Iodide of methyl.....	15·8	60	71
Benzol.....	17·5	78	79
Chloroform	17·5	89	79
Iodide of ethyl	21·5	94	97
Wood-spirit.....	26·5	123	120
Methyl alcohol	29·0	133	131
Chloride of amyl.....	30·0	137	135
Amylene	31·8	157	143

The last column gives the values of the first column multiplied by 4·5. Of these nine substances, we find three for which the results deviate considerably from proportionality. Thus, the number for iodide of methyl in the last column differs by 16 per cent. from that in the middle column, the numbers for chloroform by 11 per cent., and for amylene by 9 per cent. If three out of nine substances (that is, one third) do not show this proportionality, it is to be expected that, by increasing the number of substances experimented upon, other deviations would be brought to light—deviations which might make it impossible to maintain the theory of proportionality.

Tyndall himself singles out the following cases:—

"With the blackened tube the order of the following substances, beginning with the lowest, was

Alcohol, Sulphuric ether, Formic ether, Ethyl propionate,

whereas with the polished tube the order was

Formic ether, Alcohol, Ethyl propionate, Sulphuric ether"

—thus altogether different! But we find in other tables values for the absorption of these substances in a polished tube at 0·5 inch pressure as follows:—

Formic ether (Table XIV. p. 32) . . .	133
Alcohol (Table VII. p. 33)	157‡
Ethyl propionate (Table XV. p. 32) . .	168
Sulphuric ether (interpolated)	180

possible [The reader is requested to bear in mind that, although in " ", the words used are not precisely those employed by Dr. Tyndall, but a literal translation of the German version.—Eds. Phil. Mag.]

* The numbers under "absorption per 100" are obtained from the deflection of the galvanometer, a deflection of 1° being taken as unit. Elsewhere "absorption per 100" denotes the percentage of the total radiation.

† According to Table VI. p. 27, we should have expected 14·8 instead of this number.

‡ At the place cited the number is given as 175, which must certainly

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This last number (180) is interpolated from Tyndall's experiments, and intentionally taken as unfavourably for our case as possible—that is to say, as small as possible. In order that sulphuric ether should stand before formic ether, the deflection of the galvanometer must have fallen from 180 below 133. If we take the total radiation as (unfavourably) small as possible (say at 300), then we find that even with an absorption of 50 per cent. the result may vary about 30 per cent. of its value, accordingly as we employ blackened or polished tubes.

The numbers given above are values actually measured, and moreover with the full accuracy of which the method is capable.

Tyndall believes that the reversal of the order may perhaps be explained by the impurity of the substances employed with the blackened tube; but it is clear that we have nothing to do with impure substances and their absorption. But that this explanation is not correct is seen from later experiments, where Tyndall employs glass tubes instead of tubes of brass. Now, since glass is "much weaker in reflecting-power than brass"*, the less-powerfully reflecting glass tube ought to behave nearly as a partly blackened metallic tube. And, in fact, Table III. of the second memoir gives exactly the same order for the quantities of heat absorbed as we have previously quoted for the half-blackened metallic tube.

Exact calculations in this direction are not possible, since unfortunately Tyndall only very seldom gives the total radiation. *We believe, then, that we have shown, from Tyndall's own numbers*†, *that the absorption of radiant heat found when a polished metallic tube is employed is different from that obtained when partly-blackened tubes, or tubes of glass are employed.* It follows, then, at once that neither of the above cases yields correct results, since in both reflection takes place.

How important a source of error vapour-adhesion was, can be seen from a closer examination of Tyndall's method of measuring the vapour admitted into the tube. The arrangement to fill the large experimental tube with vapour was shortly as follows:—A glass tube was cemented onto the experimental tube, which carried a small chamber (which we will call M) closed by two taps. One tap allowed communication to be made with the large tube, the other with a space in which the fluid to be experimented upon was contained without air. Evidently, when this tap is open and the upper one closed, M

be a misprint (see Phil. Mag. (4) xxii. p. 192, where the right number is found). It should be 157; for this value corresponds to a deflection of 60° (see Tyndall, p. 29). Also p. 157: "For the usual pressure of 0·5 inch, alcohol absorbs exactly twice as much heat as benzol." Benzol = 78 (Table XII. p. 31), and $2 \times 78 = 156$.

* *Loc. cit.* p. 71. † *Loc. cit.* p. 78.

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will become filled with vapour. If the tap leading to the fluid be closed and the other opened, communication is made between M and the large tube, and the vapour contained in M expands throughout the whole tube. This process may be repeated as often as desired; and so the tube may be gradually filled with continually increasing quantities of vapour. The ratio in which the vapour contained in M expands when it comes into the large tube may be obtained from the cubic contents of the latter, which Tyndall gives incidentally as 220 cubic inches. For the sake of clearness, we will take an example (that of ether-vapour) in Tyndall's own words. Let us assume that M contained $\frac{1}{1000}$ cubic inch:—"The vapours, on entering the tube, have only the tension corresponding to the temperature of the laboratory, viz. 12 inches. This must be multiplied by 2·5 in order to give the atmospheric pressure. If, then, the $\frac{1}{1000}$ cubic inch, whose absorption, as shown, can be measured, expands into a space of 220 cubic inches, it would have a tension of

$$\frac{1}{220} \times \frac{1}{2{\cdot}5} \times \frac{1}{1000} = \frac{1}{500000}$$

of an atmosphere!"

To examine the accuracy of this method, we will choose as examples sulphide of carbon and benzol, because the tables admit of a control for these substances.

From the table for sulphide of carbon we take:—

(Table VI.) Sulphide of Carbon. Unit volume = $\frac{1}{2}$ cubic inch.

Volumes .	1·0	2·0 . . .	7·0	8·0	9·0	13·0	14·0	15·0
Absorptions	2·2	4·9 . . .	13·8	14·5	15·0	17·5	18·2	19·0

Moreover, Tyndall gives for the absorption at a mercury-pressure of $\frac{1}{2}$ and 1 inch the numbers 14·8 and 18·8.

Now there can be no doubt of the conclusion that, whenever equal absorptions take place, there must be present equal numbers of molecules of vapour, and hence at the same temperature there must be equal vapour-pressures. At $\frac{1}{2}$ inch pressure the absorption was 14·8; and the same absorption takes place, according to the table, when M has been emptied into the large tube about 8·6 times; and there must then have been in the tube a pressure of $\frac{1}{2}$ inch. From this we can easily calculate what the pressure in M must have been each time for this result to have been obtained.

If x denote the pressure in M, then when M is put into communication with the large tube there will be a pressure of $\frac{x}{220}$ in the tube, since the vapour expands into a space 220 times its own volume, supposing M to have a volume of

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1 cubic inch; but as in our case the volume of M is only $\frac{1}{2}$ cubic inch, the volume of the tube is 440 times that of M, and the vapour must have expanded into 440 times its volume; so that the pressure becomes $\frac{x}{440}$. But after a repetition of the process 8·6 times (that is, after the pressure in the tube has become $\frac{x}{440} \times 8{\cdot}6$), the total pressure in the tube, according to the tables, had become half an inch of mercury. We have therefore the equation

$$\frac{x}{440} \times 8{\cdot}6 = \tfrac{1}{2} \text{ inch, or } x = 25{\cdot}6 \text{ inches mercury.}$$

If we make the same calculation with the numbers which correspond to a pressure of 1 inch in the table, we find x from the equation

$$\frac{x}{440} \times 14{\cdot}8 = 1, \text{ or } x = 29{\cdot}9 \text{ inches mercury.}$$

We see, then, that there must have been a vapour-tension of sulphide of carbon amounting to an entire atmosphere. But as this is only possible at 46°, we have discovered some error, the cause of which must be sought for.

For benzol the calculations are still more surprising. The table gives us the following data :—

(Table XII.*) Benzol. Unit volume = $\frac{1}{10}$ cubic inch.

Volumes .	1 . . .	18·0	19·0	20·0
Absorptions	4·5 . . .	67·0	69·0	72·0

Further, the absorption was 78 at a mercury-pressure of $\frac{1}{2}$ inch.

If we extrapolate a little, we find that if M were emptied 22·5 times into the tube, an absorption of 78 would be obtained, which corresponds to the absorption at $\frac{1}{2}$ inch mercury. A similar calculation to the former one gives

$$\frac{x}{2200} \times 22{\cdot}5 = 0{\cdot}5, \text{ or } x = 99{\cdot}3 \text{ inches mercury ;}$$

that is to say, the temperature must have been about 100° C.

The explanation of these impossibilities may perhaps be as follows. Regnault† has observed that the tension of vapours is less *in vacuo* than in a space filled with air, and he explains this as the result of condensation on the walls; this causes a diminution of the tension of the vapour; so that, whilst in a vacuum compensation is instantly made by the liquid, in a space filled with air this requires time, and the full vapour-tension is never reached. In the same way the results of the above calculations may be explained.

* *Loc cit.* p. 31. † Pogg. *Ann.* xciii.; *Mémoires de l'Acad.* t. xxvi.

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When vapour forms in the empty chamber M from the liquid contained in the space below it, a portion condenses on the walls and runs down unseen. When the chamber is shut off from the liquid, therefore, it does not contain only vapour, but also the liquid which has condensed on the walls. Hence when communication is made with the large tube, not only does the vapour diffuse itself through the tube, but the liquid condensed in M also evaporates. There can be no doubt that condensation takes place in the large tube also; but since the evaporation is limited, equilibrium is attained. Hence it seems to us we have an explanation of the results of the above calculations; and at the same time we see that vapour-adhesion is so important a source of error in Tyndall's experiments that all his results must be considered doubtful. Still further, in his later experiments Tyndall changed the method of introducing the vapour into the tube, allowing it to enter directly from a small flask in which the liquid was contained free from air, the tension being measured by a mercurial manometer. The enormous condensation which we have previously found on the walls of the small chamber M, now takes place in the large tube.

Another inaccuracy must be pointed out in Tyndall's method of dealing with percentages. It is the result of the method of compensation which he employs. The sensitiveness which is attained by giving the absorption in units of the galvanometer, the first degree =1, is more than compensated by the inaccuracy in determining the deflection produced by the total heat in these units, since these deflections amount to 70° or 80°, and one degree in this position is equal to about 20 units.

Possibly this is the reason why Tyndall scarcely ever gives the deflection produced by the total heat. His tables hardly ever permit of our obtaining in any case of interest any thing else than the ratio of absorptions in such a case; so that it is impossible to express the absorptions in percentages.

We are acquainted with only one other research on the absorption of radiant heat by vapour, besides the numerous researches of Tyndall in this direction, that, namely, of Hoorweg*. This experimentalist endeavoured to avoid vapour-adhesion by allowing a stream of vapour to rise freely in the room between the source of heat and the thermopile. It is obvious that as a *quantitative* determination this method must yield less accurate results than that which we shall immediately describe. Moreover, Hoorweg's chief object was to observe the behaviour of aqueous vapour, and his results are much less in contradiction with ours than are those of Tyndall. We shall

* Pogg. *Ann.* clv.

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return to this subject in speaking of the absorption of aqueous vapour.

Description of the Apparatus employed and of the Methods of Research.

Guided by the considerations already given, and taught by many failures, we finally arranged method and apparatus as follows:—It was necessary to do away with rock-salt plates and all reflection, and yet to have the thermopile as well as the radiating-surface which served as source of heat in the same experimental space, so that there should be no conduction and no currents of air. We believe, then, we have solved this problem by means of a piece of apparatus which is represented in the accompanying plate on a scale about one tenth of the actual size.

A is the special experimental vessel, an inverted glass bell-jar with the open end upwards. The thermopile S is placed on the bottom of this glass vessel on a wooden support *b*, and is without conical reflector or any similar arrangement for collecting the rays. Its upper face, carefully covered with lamp-black, is directed upwards towards the source of heat. The lower face is protected by a packing of cotton-wool against any possible sudden change of temperature. The aperture of the vessel A is closed air-tight by a special vessel B, which carries an arrangement for radiating heat to the thermopile. This consists of a bottom plate of metal *c*, in the centre of which there is a round hole in which a metallic tube *l*, 3·5 centimetres in diameter, is soldered. In this tube, at about one fifth of its height, there is a thin metal plate *a*, which, as well as *l*, is carefully covered with lamp-black. The distance from *a* to S was 310 millimetres. Into the bottom plate *c* a second tube, *i*, is soldered, into which the india-rubber stopper (which carries the manometer-tube *e*, the conducting-wires, and the tube *r* leading to the air-pump) is cemented air-tight. Besides these, the plate *c* carries a glass ring *h*, cemented round its outer edge, which is open at both sides and enables a current of water to circulate constantly about the tube *l*. The whole vessel A was placed in an ice-calorimeter C, as used by Lavoisier and Laplace. The water from the water-tap *q* flows into the inner space round the vessel A, then flows over into the outer space and runs off by the tube *s*. This arrangement ensures that the whole of the vessel A shall be surrounded by water always of the same temperature; so that the thermopile is always exposed to equal radiation from the glass walls of A. The tap *p* for the upper cooling vessel-terminates in a leaden tube which surrounds the tube *l*, and is pierced with

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holes like a sieve; so that *l* is continually exposed to a current of water, which flows over the edge of *h* and runs off by the tube *s*. It need hardly be remarked that the temperature of the water was observed from time to time. In the course of months a slight change occurred; but during the time occupied by an experiment the temperature was absolutely constant.

After the temperature of the whole has become uniform, the little plate *a* is suddenly to be increased in temperature by a certain amount. We sought to accomplish this by suddenly directing a current of steam against *a*. In the arrangement of the heating-apparatus it was necessary above every thing to consider that the thin copper plate *a* in a series of experiments must always be equally heated. But since, in consequence of the close neighbourhood of the large quantity of cooling-water, the loss of heat by conduction was considerable, it was only possible to secure equality in the radiation towards the thermopile (which was to be suddenly set in action) by always directing upon *a* equal quantities of vapour with equal velocities, and also in the same direction and at the same distance from *a*. Hence it was necessary that equal quantities of vapour should be generated in equal times. Hence, when ebullition commences, there must always be nearly the same quantity of water in the apparatus M employed for the generation of steam, and the flame should always be of the same size. The first object was attained by employing the arrangements for keeping M filled with water to the same level represented in the plate. The flask K, about five sixths full of water and inverted over the funnel T, has two tubes, passing air-tight through an india-rubber stopper, which dip below into the water in the funnel, and which terminate at different levels inside the flask, one just inside the stopper, the other reaching into the air in the upper part of the flask. This arrangement, as is well known, secures a constant level in the water in T.

Each time, on beginning to heat the vessel M, the tube was closed at *o* by means of a clamp, which was opened again at the end of the experiment. In order to have a constant source of heat under M, a manometer, *m*, containing petroleum, was used to measure the pressure of the gas supplying the burner *z*, the current of gas being regulated by means of the screw-clamp *y*. Another manometer, *d*, indicated accidental excess of pressure in M. The production of vapour was allowed to become so rapid that the steam issued briskly from the end of the tube *v*. This tube began and ended in glass tubes which were connected by a piece of caoutchouc tubing; and the whole was wrapped round with linen to prevent premature

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condensation of the vapour. The whole flexible tube ended in the stopper *w*, by means of which it could be adjusted in the brass tube *l*, as shown in the plate at *w'*. The stopper has incisions in it at the sides, through which the steam escaped after impinging upon the little plate *a*. The whole of the apparatus for the production of vapour was separated from the apparatus A B C by means of screws not shown in the plate.

The pressure in A could be read off on the mercurial manometer R. The air-pump D, a good Ekling's pump, served for the exhaustion of A. In the experiments with vapours the tube N, arranged after the fashion of a wash-bottle, was employed, which contained the liquid the vapour of which was to be examined, and which was placed in a bath connected with the water-tap, so that its temperature might be kept constantly the same as in the vessel A. In this way the precipitation of the vapour in the vessel A was avoided. Dry and pure air was allowed to pass slowly through the fluid in small bubbles, so as to ensure its saturation with vapour at the temperature of the experiment.

Whilst A could have been filled with air in about a minute and a half, we opened the tap *b* so little (*g* was closed) that it required two hours to fill A completely. The apparatus A will then be full of vapour at the maximum tension corresponding to the temperature. A small quantity may have condensed on the walls, and so reduced the tension slightly. This is, we believe, the only source of error in our method which has not been completely removed; it can, however, hardly have much influence on the results. That a little vapour may have condensed on the thermopile and on the radiating surface *a* is of no consequence, since though possibly a small part of the radiation may be absorbed by it, yet the very fine, thin layer of fluid will act like a slight increase in the layer of lamp-black.

The air destined to serve as the carrier of the vapour came from the gas-holder G, into which it was brought direct from the court of the Institute. From G it goes through several chloride-of-calcium tubes, and then through the three special pieces of apparatus, O, P, Q. In P and Q are pieces of glass the size of hazel-nuts, which have been allowed to soak for a day in boiling sulphuric acid, and have then been placed in the vessels P and Q. After repeated washing with distilled water, the corks were adjusted and made air-tight, and then dry pure air was drawn through them for three weeks by means of a Bunsen's filter-pump. Next, the whole apparatus was filled with chemically pure sulphuric acid by means of

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the siphon-shaped glass tubes. After standing 24 hours, the sulphuric acid was blown out the same way, and the end of the glass tube carefully cleaned.

In O, on the other hand, pieces of marble were placed which had been cleaned on the surface with hydrochloric acid, then washed and dried, and moistened with a solution of caustic potash. This kind of purifying-apparatus is mainly adopted from Tyndall *, and should be employed in all researches in which the chemical purity of gases is required, instead of the usual Bunsen's wash-bottles. Lastly, the tube *x* must be mentioned, which was filled with pure cotton-wool and the upper portion with asbestos, to separate the organic germs, which, according to Tyndall †, can only be separated in this way.

The galvanometer, an ordinary thermo-multiplier, the copper wire of which was covered with white silk free from iron, was placed several rooms away from the rest of the apparatus. It was observed, in fact, that the galvanometer altered its zero-point considerably with change of temperature; so that the room in which it was placed, on a small table attached to the wall, was kept with the windows closed in order to avoid change of temperature as much as possible. The instrument was provided with a mirror, and a scale at a distance of about 3 metres from the mirror. A board was placed horizontally in front of the the scale, on which a small gas-flame could be moved by the observer at the telescope by means of cords, so as to adjust the illumination of the scale. In this way the scale was easily illuminated, even when the deflection was very great, without any elaborate arrangement.

The coil of the galvanometer consisted of copper wire 3·5 millims. thick. No binding-screws were employed. At the points where this wire was to be connected with the wires leading from the thermopile, the wires, after being carefully polished, were twisted together; then the joint was carefully varnished and plunged in water coming from the tap, so as to prevent the production of any thermo-current.

One of the two conducting-wires was completely insulated; for we had occasion to confirm an observation already made by Lamont ‡, that perfectly dry wood is no insulator for feeble currents.

* *Loc. cit.* p. 74. † *Loc. cit.* Memoir, 10, p. 341.

‡ Lamont, *Der Erdstrom*, Leipzig (published by Voss), p. 7. When the one wire of our galvanometer was connected with the water-pipes, and the end of the other (insulated according to ordinary views) was held in the hand, a considerable current was produced in the apparently open circuit, producing a deflection of nearly 100 divisions of the scale.

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The method of conducting the experiments was as follows:—After the quantity of water in the heating-apparatus and the pressure of the gas had been adjusted and the water made to boil rapidly, one of us observed the galvanometer, whilst the other held himself ready to adjust the tube through which the steam was issuing in the tube *l*. At a signal from the observer at the galvanometer the steam-tube was put into its place. The position of rest of the galvanometer was observed at the moment of giving the signal; the current produced drove the needle away; and the first deflection and three reversal-points besides were noted. The whole observation occupied about 1½ minute. We believe that in this way we have completely avoided all errors resulting from air-currents produced. In fact, it could be seen from intentionally protracted observations, of which we made many for the purpose, that when the maximum was reached at the time of the third reversal-point it remained constant at the time of the fourth, but that generally a slight increase could be observed in the time from the third to the fourth. Not until the sixth, and sometimes not until the eighth reversal, did the air-currents become perceptible. The logarithmic decrement for the vibrations of the pair of needles was determined as often as possible; and the true deflection calculated from the three reversal-points, after the first deflection had taken place, by means of the formula

$$\text{deflection} = \frac{(p_2+p_3)+k(p_3+p_4)}{2(1+k)} - \mathrm{R},$$

where k is the ratio of the amplitudes of two consecutive vibrations, R the original position of rest, and p_2, p_3, p_4 the reversal-points. The first deflection is not employed, since the rapidity with which the steam-tube was placed in l and similar accidents might affect it.

Calculation showed that it was unnecessary to reduce parts of the scale to corresponding arcs. After each experiment was concluded in the manner described, the tube v was brought again into its original position, and by opening the clamp o for a time the original level of the water was restored; the small space above a was also carefully dried.

A whole series of experiments was so arranged that the first three experiments were made *in vacuo* at intervals of 30 minutes. Then the galvanometer was put out of the circuit by connecting the conducting-wires with each other by a copper bridge so that A could be exhausted. If the galva-

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nometer had remained in the circuit, the cooling of the thermopile by the rarefaction of the air in A would have produced a powerful thermo-current injurious to so delicate a galvanometer. After A had been filled with the gas or vapour to be investigated an interval of two hours was allowed, so that its temperature, which had been raised by the gas rushing into it, might have sunk again to that of the surrounding water. Then the separate experiments were made again, in the same manner as already described.

As some of the more extended series of experiments required 12 to 14 hours, and so long a time was seldom at our disposal during the day, we almost always made our experiments at night—a plan which had other advantages also, in consequence of the very unfavourable position of the Institute. The vibration from the crowded street visibly affected the galvanometer in the daytime; and its position of equilibrium was much more constant at night.

Results of Experiments.

I. *Pure dry Air.*

The absorption of heat by this mixture of gases is of the highest physical interest, especially in questions of radiant heat. The results we obtained at first differed very much among themselves; and it was not until we had learned to take all the precautions which have been described that we obtained agreeing results. Of the large number of our experiments, in part of which snow was employed as cooling-agent, we give two series.

In all the following Tables, R_0 denotes the position of rest of the galvanometer before the heating took place, R_1 the calculated position of rest after steam had been allowed to stream against the little plate *a*; J gives the radiation which has passed through the medium, the intensity of the original radiation being put equal to 100; *k* denotes the ratio of two consecutive amplitudes, and *t* the temperature of the water-bath in centigrade degrees.

It must be remarked, with reference to the arrangements of this and following Tables, that, in order to save space, two quite different series of experiments are often separated only by a line.

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	Name and Pressure of gas.	B_0	Reversal-points. p_1.	p_2.	p_3.	p_4.	B_1	Deflection in divisions of the scale.	Mean.	J.
$k=1{\cdot}9.$ $t=6°{\cdot}8.$	Pure air at atmospheric pressure.	66	663	543	660	608	622·8	556·8	557·1	100·08
		66	675	538	666	608	624·9	558·9		
		66	682	534	664	603	621·6	555·6		
	Vacuum.	66	650	540	658	611	622·3	556·3	556·7	100·00
		67	652	541	662	610	624·1	557·1		
		67	657	538	662	610	623·6	556·6		
	Pure air at atmospheric pressure.	65	682	531	655	605	617·2	552·2	553·8	99·49
		66	684	531	654	615	619·5	553·55		
		68	677	534	663	611	623·7	555·7		
	Vacuum.	66	648	537	656	609	620·1	554·1	553·9	99·50
		66	647	538	655	609	619·6	553·6		
		64	647	535	653	608	617·9	553·9		
$k=1{\cdot}9.$ $t=6°{\cdot}8.$	Pure air at atmospheric pressure.	65	674	542	648	612	616·6	551·1	551·3	100·14
		65	682	543	654	603	618·6	553·1		
		66	690	530	655	602	616·1	550·1		
	Vacuum.	66	661	544	650	605	617·0	551·0	550·5	100·00
		65	659	539	650	603	615·5	550·5		
		64	658	537	648	602	613·9	549·9		
	Pure air at atmospheric pressure.	63	670	538	645	596	610·5	547·5	548·1	99·56
		64	670	538	648	600	613·3	549·3		
		66	688	532	650	601	613·6	547·6		
	Vacuum.	65	660	540	650	603	615·6	550·6	550·4	99·90
		65	661	539	651	604	616·3	551·3		
		64	658	535	651	600	613·3	549·3		

We found that in course of time the deflections on our galvanometer became continually smaller, partly because the needles became less nearly astatic, but chiefly because the points of contact at the ends of the conducting-wires became oxidized in spite of the careful varnishing which they had received, and thus the resistance was increased.

These changes were of course imperceptible during any one series of experiments; but they explain the differences observable in the deflections of two series of experiments, which may be printed the one under the other, though possibly there may have been actually an interval of some months between them.

As far as the Table given above is concerned, it clearly appears that the absorption of heat-rays from a source at 100° C. by a layer of 31 centimetres thickness, is so small that it cannot be measured; for if we take the mean of the first three experiments and

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of the third three, and compare with the middle three, and so also of the second and fourth with the middle three, we obtain for J:—

In vacuo...	100·00	99·95	100·00	99·75
In air	99·85	99·56	99·78	99·49

or, as mean result, that of 100 incident rays 99·78 pass through. The difference between these numbers is unquestionably not more than the unavoidable errors of observation.

II. *Moist Air.*

	Name and pressure of vapour.	R_0.	Reversal-points.				R_1.	Deflection in divisions of the scale.	Mean.	J.
			p_1.	p_2.	p_3.	p_4.				
$t=6°·8$. $k=1·9$.	Pure dry air at atmospheric pressure.	66	673	524	655	590	611·1	545·1	545·8	100·00
		65	662	525	654	589	610·5	545·5		
		63	661	523	650	594	609·7	546·7		
	Aqueous vapour 7·4 millims.	64	688	523	658	592	613·4	549·4	549·7	100·71
		65	695	523	660	590	613·4	548·4		
		66	698	526	663	596	617·4	551·4		
	Pure dry air at atmospheric pressure.	64	693	520	661	594	614·7	550·7	551·0	100·95
		67	693	525	663	596	617·3	550·3		
		65	694	524	661	599	617·1	552·1		
	Aqueous vapour 7·4 millims.	66	696	524	664	594	616·9	550·9	550·4	100·84
		65	684	525	661	593	615·3	550·3		
		66	683	527	660	596	616·1	550·1		
$t=6°·8$. $k=1·9$.	Aqueous vapour 7·4 millims.	66	693	520	660	583	610·5	544·5	545·2	99·87
		64	692	521	658	582	609·6	545·6		
		69	701	528	663	587	614·5	545·5		
	Pure dry air at atmospheric pressure.	63	693	522	658	583	610·0	547·0	545·8	100·00
		62	680	525	655	583	609·0	547·0		
		62	686	518	654	577	605·3	543·3		
	Aqueous vapour 7·4 millims.	66	697	526	660	579	610·3	544·3	546·4	100·11
		69	707	531	672	587	619·8	550·8		
		66	697	525	660	579	610·2	544·2		

Arranged in the same way as before, we have for J:—

Air	100·00	100·00	100·00
Aqueous vapour...	100·24	100·17	99·99

or, as mean result, that of 100 incident rays 100·20 emerge. Hence the result of these experiments is that moist air does not perceptibly absorb the heat-rays from a source at 100° C. No imaginable source of error has here been left out of account. The arrangement for filling with moist air was varied, the air allowed to stand for a long time over the water in the

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gas-holder, and this moist air then passed through several wash-bottles into the experimental space, but with the same *negative* result. This result was so much the more surprising, since all experiments show absorption of the heat-rays of the sun by the atmosphere, which, since it cannot be ascribed to the pure air, has universally been put down to the account of the aqueous vapour.

In opposition to this result is that obtained by Tyndall, who found an absorption of 4–6 per cent.* with a source of heat at 100° C., as also at higher temperatures.

It was shown above that vapour-adhesion must take place on the rock-salt and on the polished inner walls of the tube. To this we have nothing more of importance to add; only the remark may be allowed that Tyndall himself does not consider his experiments in air, without a tube, decisive, and that the reflectors of the thermopile were allowed to remain, thus partially replacing the tube—an objection which applies also to Hoorweg's† work.

We would express in the form of a question a thought which has frequently occurred to us during the study of this subject. Why did not Tyndall take the simplest way of setting aside the objections of Magnus, and simply use a blackened tube provided with diaphragms instead of the polished one? Why did he content himself with making *one* experiment (in a case so readily open to objection) with a half-blackened tube, and then take his stand upon the proportionality of the action? The matter seems so simple that we have never been able to understand why Tyndall did not experiment with blackened tubes.

A statement which will possibly be quite convincing we leave till further on.

In what follows we give the results of our experiments on gases and vapours.

As far as the first are concerned, our numbers differs very little from those of Tyndall; whilst with vapours we have almost always obtained different values. Many of them were

* *Loc. cit.* pp. 133, 134.

† *Loc. cit.* p. 131. The experiments repeated by Frankland, at Tyndall's request, lead to the same result (*loc. cit.* p. 183). For air saturated with aqueous vapour at a temperature of about 12° C. he gives 5½ per cent. Hoorweg (Pogg. *Ann.* clv.) found also an absorption for aqueous vapour, though only about half as much as Tyndall gives. The method of allowing air saturated with aqueous vapour to rise freely does not appear to us to be free from objection. Would not invisible globules of water form there very easily? and would the walls of the reflectors and of the thermopile actually remain unaffected?

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measured with the same minute accuracy and care as the two already given, since we hoped to draw conclusions on the relation between pressure and absorption. Nevertheless, from the experiments with ether vapour, carbon dioxide, &c. this much may be concluded with certainty, that the well-known law of absorption $J = 100\epsilon^{-dx}$ does not apply here, since the coefficient x always becomes smaller as the thickness d of the layer becomes smaller. Hence the absorption is selective even with a source of heat at 100° C.

The results will be discussed at the end of this paper.

I.

	Name and pressure of vapour.	R_0.	Reversal-points. p_1.	p_2.	p_3.	p_4.	R_1.	Deflection, in divisions of the scale.	Mean.	J.
1. Methyl Alcohol.										
$k=1{\cdot}8$. $t=8°{\cdot}15$.	Pure dry air.	377	745	631	685	654	665·4	288·4	289·4	100·00
		375	739	635	682	655	664·6	289·6		
		388	751	651	697	664	678·2	290·2		
	CH_4O, 44 millims.	366	730	623	674	641	654·4	288·4	288·9	99·82
		370	738	630	681	647	660·9	290·9		
		370	734	624	677	645	657·3	287·3		
$k=1{\cdot}8$. $t=8°{\cdot}5$.	Pure dry air.	221	440	368	343	384	360·6	139·6	138·7	100·00
		220	438	368	340	380	357·8	137·8		
		219	433	368	339	381	357·6	138·6		
	CH_4O, 44 millims.	217	431	364	338	377	355·2	138·2	137·8	99·42
		218	433	366	338	379	356·1	138·1		
		217	433	363	337	376	354·2	137·2		
2. Formic Acid.										
$k=1{\cdot}8$. $t=8°{\cdot}8$.	Pure dry air.	325	565	486	519	498	506·5	181·5	181·6	100·00
		326	571	493	520	497	507·7	181·7		
		324	569	489	514	496	505·7	181·7		
	CH_2O_2, 145 millims.	327	564	483	520	495	505·4	178·4	179·0	98·55
		326	567	486	518	496	505·2	179·2		
		326	565	484	520	495	505·5	179·5		
3. Chloroform.										
$k=1{\cdot}8$. $t=8°{\cdot}8$.	Pure dry air.	320	559	476	514	489	499·2	179·2	179·3	100·00
		322	560	479	515	492	501·2	179·2		
		319	561	474	513	490	498·6	179·6		
	$CHCl_3$, 70 millims.(?)	315	550	470	508	483	493·2	178·2	178·4	99·50
		320	560	477	513	491	499·5	179·5		
		320	561	476	511	489	497·6	177·6		

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4. Carbonic Oxide.

	Name and pressure of vapour.	R_0.	Reversal-points.				R_1.	Deflection, in divisions of the scale.	Mean.	J.
			p_1.	p_2.	p_3.	p_4.				
$t=8°{\cdot}5$.	Pure dry air.	354	830	706	769	737	747·5	393·5	393·9	100·00
		370	881	734	787	751	766·0	390·0		
		358	877	716	779	741	755·3	397·3		
	CO, 745 millims.	380	852	718	769	737	749·6	369·6	371·5	93·90
		359	820	693	750	719	729·9	370·9		
		368	838	701	763	732	742·0	374·0		
$k=1{\cdot}8$.	Pure dry air.	345	829	707	763	734	743·6	398·6	400·8	101·31
		350	831	709	770	739	749·1	399·1		
		360	850	719	794	745	764·9	404·9		
$t=8°{\cdot}5$.	Pure dry air.	365	860	702	772	727	745·3	380·3	377·7	100·00
		373	855	707·5	776·5	731	749·6	376·6		
		375	815	714	772·5	739	751·3	376·3		
$k=1{\cdot}8$.	CO, 743 millims.	382	771	701	747	725	731·7	349·7	350·9	92·90
		370	758	687	787	713	720·3	350·3		
		373	754	693	742	719	725·8	352·8		

5. Carbonic Acid.

$t=8°{\cdot}0$	Pure dry air.	385	738	621	673	637	652·2	267·2	266·8	100·00
		395	743	631	681	648	661·6	266·6		
		379	721	613	662	631	643·4	264·4		
	CO_2, 748 millims.	395	710	608	656	630	639·2	244·2	245·2	91·90
		367	685	587	631	602	613·9	246·9		
		390	718	614	662	634	644·5	244·5		
$k=1{\cdot}9$.	CO_2, 8 millims.	380	723	614	663	630	643·6	263·6	264·9	99·29
		392	740	627	677	643	657·1	265·1		
		390	740	625	676	642	655·9	265·9		
$t=6°{\cdot}8$.	CO_2, 750 millims.	67	634	493	615	549	572·3	505·3	506·6	92·40
		68	634	500	616	549	574·1	506·1		
		65	637	494	613	555	573·5	508·5		
	CO_2, 340 millims.	68	637	505	625	573	587·3	519·3	517·5	94·41
		67	637	504	624	565	583·8	516·8		
		66	639	498	624	563	582·3	516·3		
$k=1{\cdot}9$.	CO_2, 190 millims.	64	641	511	632	569	590·5	526·5	527·5	96·24
		67	644	515	638	573	595·5	528·5		
		67	647	511	636	575	594·5	527·5		
	Pure dry air.	67	688	570	661	595	615·1	548·1	548·1	100·00
		65	686	525	660	589	613·3	548·3		
		66	676	521	655	596	614·0	548·0		

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II.

	Name and pressure of vapour.	R_0.	Reversal-points. p_1.	p_2.	p_3.	p_4.	R_1.	Deflection, in divisions of the scale.	Mean.	J.
1. Ethyl Alcohol.										
$k=2{\cdot}11$. $t=8°{\cdot}05$.	C_2H_6O vapour, 20 millims.	3·0	836	593	754	675	701·3	698·3	697·8	93·50
		7·5	845	592	761·5	674	705·0	697·5		
		7·0	843	604	749	687	704·7	697·7		
	C_2H_6O, 7 millims.	7·0	848	624	783	713	733·6	726·6	725·8	97·25
		10·0	845	631	784	709	734·6	724·6		
		12·0	851	630	789	715	738·3	726·3		
	C_2H_6O, 2·4 millims.	9·0	844	640	777	739	742·0	733·0	734·9	98·47
		7·0	850	643	794	715	743·2	736·2		
		11·0	856	644	798	719	746·4	735·4		
	Pure dry air.	13·0	885	661	813	729	760·0	747·0	746·3	100·00
		5·6	875	647	798	725	748·9	743·3		
		14·0	381	658	813	738	762·6	748·6		
$k=2{\cdot}1$. $t=8°{\cdot}25$.	C_2H_6O, 22 millims.	13	979	625	787	698	730·7	717·7	716·3	93·49
		12	874	616	780	698	725·8	713·8		
		13	877	622	780	709	730·5	717·5		
	C_2H_6O, 10·0 millims.	12	883	647	802	723	750·2	738·2	742·1	96·85
		13	885	649	805	735	756·6	743·6		
		14	890	650	808	737	758·5	744·5		
	C_2H_6O, 3·5 millims.	12	885	664	820	743	768·8	754·8	753·2	98·30
		8	872	661	803	746	760·8	752·8		
		11	882	660	815	738	763·9	752·9		
	Pure air.	5	918	670	807	764	770·3	765·3	766·2	100·00
		13	898	685	822	762	779·5	766·5		
		14	902	684	817	774	780·7	766·7		
2. Acetic Acid.										
$k=1{\cdot}8$. $t=8°{\cdot}0$.	Pure air.	370	813	686	734	692	711·9	341·9	341·5	100·00
		374	813	691	738	698	716·7	342·2		
		380	819	695	744	699	720·4	340·4		
	$C_2H_4O_2$ vapour, 5 millims.	380	789	671	715	678	695·5	315·5	314·1	91·98
		361	767	651	690	664	674·7	313·7		
		366	772	656	694	669	679·7	313·2		
$k=1{\cdot}8$. $t=8°{\cdot}0$.	$C_2H_4O_2$ vapour, 5·1 millims.	367	755	656	695	663	677·5	310·7	311·5	92·35
		370	773	656	703	665	682·7	312·7		
		369	756	658	697	666	680·1	311·1		
	Pure air.	370	804	682	727	691	707·9	337·4	337·3	100·00
		383	817	691	744	698	719·8	336·8		
		383	818	692	745	698	720·6	337·6		

The acetic acid used was probably impure with water and alcohol. The true absorption will therefore be somewhat smaller.

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3. Ether.

	Name and pressure of vapour.	R_0.	Reversal-points. p_1.	p_2.	p_3.	p_4.	R_1.	Deflection, in divisions of the scale.	Mean.	J.
$t=7°{\cdot}45.$ $k=2{\cdot}0.$	Vacuum.	434·0	1268	1061	1183	1109	1138·4	704·0	704·8	100·00
		435·0	1270	1065	1183	1115	1141·2	706·2		
		437·0	1273	1063	1185	1113	1141·2	704·2		
	$(C_2H_5)_2O$, 12·8 millims.	437·0	1198	1022	1140	1094	1105·0	668·0	669·3	94·96
		435·0	1200	1024	1145	1091	1406·8	671·8		
		437·5	1204	1022	1144	1089	1106·3	668·3		
	$(C_2H_5)_2O$, 41·2 millims.	433	1128	958	1066	1020	1032·7	599·7	601·7	87·37
		435	1132	962	1077	1025	1040·5	605·5		
		435	1135	952	1069	1025	1035·8	600·8		
	$(C_2H_5)_2O$, 78·6 millims.	436	1072	919	1015	980	987·3	551·3	549·3	77·94
		435	1075	916	1010	975	982·7	547·7		
		436	1078	920	1015	972	984·8	548·8		
	$(C_2H_5)_2O$, 125·2 millims.	436	1001	857	946	910	919·2	484·2	488·0	69·24
		438	1007	856	958	918	927·7	489·7		
		437	1009	864	955	917	927·2	490·7		
	$(C_2H_5)_2O$, 233·3 millims.	440	915	789	884	848	856·2	416·2	412·9	58·59
		435	911	774	866	855	847·0	412·0		
		436	908	774	866	854	846·7	410·7		
$t=7°{\cdot}35.$ $k=2{\cdot}0.$	$(C_2H_5)_2O$, 255·8 millims.	61	595	452	522	500	503·0	442·0	439·3	54·00
		59	595	448	518	494	498·3	439·3		
		59	580	445	512	501	497·2	438·2		
	$(C_2H_5)_2O$, 68·5 millims.	60	797	625	716	679	688·5	628·5	630·3	77·43
		62	796	619	729	683	695·3	633·3		
		64	788	630	722	684	694·0	630·0		
	$(C_2H_5)_2O$, 19·0 millims.	56	886	700	811	765	777·5	721·5	719·3	88·32
		60	884	701	811	763	776·7	716·7		
		56	870	698	805	766	775·8	719·8		
	Vacuum.	64	1048	804	927	851	881·2	817·2	814·4	100·00
		63	1013	800	921	853	878·2	815·7		
		59	1018	789	911	849	870·0	811·0		

4. Ethylene (Olefiant gas).

$t=8°{\cdot}1.$ $k=1{\cdot}8.$	Pure dry air.	212	435	363	399	376	385·3	173·3	174·6	100·00
		201	425	354	387	366	374·6	173·6		
		208	434	363	398	377	384·9	176·9		
	C_2H_4, 751 millims.	179	295	256	276	267	269·5	90·5	90·3	51·74
		205	321	283	302	291	295·2	90·2		
		213	338	289	310	306	303 3	90·3		
	Pure dry air.	213	435	363	390	379	386·6	173·6	175·4	100·45
		200	426	354	390	369	376·9	176·9		
		201	429	355	391	370	377·8	176·8		

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III. Fermentation—Butyl Alcohol.

	Name and pressure of vapour.	R_0.	Reversal-points.				R_1.	Deflection, in divisions of the scale.	Mean.	J.
			p_1.	p_2.	p_3.	p_4.				
$k=1{\cdot}8$. $t=8°{\cdot}5$.	Pure dry air.	447	746	656	692	665	676·9	229·9	229·3	100·00
		448	741	653	690	670	677·4	229·4		
		449	741	651	691	671	677·7	228·7		
	$C_4H_{10}O$, 3 millims. (?)	445	701	624	656	637	644·2	199·2	202·4	88·27
		454	715	632	670	651	657·1	203·1		
		448	713	633	665	645	652·9	204·9		

Purification by fractional distillation was not possible, because of the small quantity of the substance at our disposal.

IV. Fermentation—Amyl Alcohol.

$k=1{\cdot}9$. $t=8°{\cdot}5$.	Pure dry air.	6	214	142	175	153	162·1	156·1	155·3	100·00
		6	208	139	173	153	160·6	154·6		
		5	206	140	171	154	160·3	155·3		
	$C_5H_{12}O$, tension (?).	6	200	135	168	147	155·4	149·4	151·2	97·36
		6	204	140	169	149	157·4	151·4		
		2	199	138	166	147	154·9	152·9		

V. Benzol.

$k=1{\cdot}8$. $t=8°{\cdot}5$.	Pure dry air.	445	796	690	746	714	725·8	280·8	279·6	100·00
		445	799	697	746	710	725·8	280·8		
		478	830	730	774	740	755·7	277·2		
	C_6H_6, 42 millims.	462	797	690	747	713	725·9	263·9	262·3	93·81
		440	780	670	722	687	700·8	260·8		
		458	802	691	739	707	720·1	262·1		

Conclusions.

We have already remarked more than once how great an importance attaches to the investigation of the absorption of radiant heat by gases and vapours in reference to meteorological questions. As far as aqueous vapour is concerned, since the first discussion of this subject by Tyndall until now, there has been no hesitation in ascribing that which has been measured only for a small number of wave-lengths to other wave-lengths, or even to the whole spectrum. Two generalizations have been made—one to the absorption of the radiation of the earth, the other to the absorption of the radiation of the sun by our atmosphere.

As far as the radiation of the earth is concerned, we have

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here to do with wave-lengths which are almost all greater than those which have been examined in the laboratory.

Hence all conclusions and remarks in this direction are only admissible on the assumption that the absorption-ratios found hold good for the neighbouring less-refrangible region of the spectrum; and this, however probable it may appear, has never been proved.

The opposite generalization, to the case of the absorption of the sun's radiation is just as little justifiable; only here there is somewhat more reason for the proceeding, since some of the wave-lengths in the sun's radiation coincide with those examined by Tyndall. That portion of the total solar radiation must of course first be found which is caused by waves of shorter length than those of Tyndall's source of heat (270°). It is at present only possible to make a very rough estimation of this ratio, which may perhaps be reached as follows:—The incandescence of a body begins, according to Draper*, at a temperature of 525° C. In order to find the ratio between the radiation at 270° C. and at 525° C. we will employ a law which Stefan† has stated as the result of all the experiments made in this direction.

The quotient is found to be

$$(273+270)^4-(273)^4 : (273+525)^4-(273)^4;$$

that is, as 1 : 4. The radiation of the body increases, therefore, in the ratio of 1 to 4 when its temperature is increased from 270° to 525°; but the greater radiation is partly due to increase in the intensity of the old wave-lengths. We have therefore the discrepancy that at least the fourth part of the radiation of a blackened surface at 525° C. is due to wave-lengths which were already present at a temperature of 270° C. Now Wm. W. Jacques‡ has shown that from the temperature of low redness up to the most intense white heat the relation of the energy of radiation is nearly the same. Consequently, at least the fourth part of the dark radiation must be produced by rays such as Tyndall investigated. If now we assume, in accordance with the experiments of Müller§, that the dark portion of the sun's radiation is only twice as great as the luminous portion, we shall have at least $\frac{2}{3} \times \frac{1}{4}$ of the intensity of the total solar radiation due to such wave-lengths as Tyndall employed. (If the emission-coefficient of the sun were not equal to unity, the above fraction

* Phil. Mag. (4) xxx.
† *Sitzungsberichte*, lxxix. Abth. 2.
‡ Proceedings of the American Academy, new series, vol. vi. 1879.
§ Pogg. *Ann.* cv.

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would only have to be slightly altered, as may be seen from the research of Jacques.) The discrepancy will be still further increased by the improbable assumption that absorption takes place only in the part of the spectrum investigated, and that therefore the percentages of absorption found are distributed over the whole higher portion of the spectrum without undergoing increase there.

When, therefore, Tyndall finds for any medium an absorption of x per cent. by use of a source of heat at 270° C., the same length of the same medium must absorb at leat $\frac{x}{6}$ *per cent. of the solar radiation.*

With the question thus simplified, the discussion of the meteorological results becomes very simple. The most accurate measurements are those of Violle*, who, as is well known, made experiments on the intensity of solar radiation on the summit of Mont Blanc and at its foot, on the Glacier des Bossons. He obtained an absorption of 16 per cent. in the intervening air.

We can easily calculate from this what absorption one metre of air such as that on the top of Mont Blanc would exert, if we employ the law of absorption, as we may certainly do for such an approximation as this.

The height of the barometer on the summit was 430 millims., on the Glacier des Bossons, 661 ; hence the intervening column of air, reduced to atmospheric pressure, would have a thickness of 2428 metres. From the formula

$$A = E\epsilon^{-d \cdot x},$$

where A denotes the emergent heat, E the incident heat, and d the thickness of the layer, in metres, x is found to be 0·00007; and hence for one metre A = 99·9930, or there is an absorption of 0·0070 per cent. But now, according to Tyndall, a column of air of one metre thickness should absorb 0·086 per cent.†, and then Violle should have found for this length at least $0{\cdot}086 \times \frac{1}{6}$, or 0·0147 per cent. Hence it follows that the absorption by air alone is sufficient to account for the absorption of the sun's rays in our atmosphere.

After what has been said, Tyndall's result, that aqueous vapour in a length of one metre absorbs 4 to 6 per

* *Compt. Rend.* 1876, i. pp. 662 & 729. The heat received from the sun amounted to 2·392 cal. per minute and square centimetre on the summit of Mont Blanc, and 2·022 at the foot of the mountain.

† This number is calculated from Table I. p. 80. There, exceptionally, it is possible to calculate the percentage, since ammonia is given as completely opaque, the corresponding number 1195 representing therefore the total radiation.

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cent., will appear altogether unintelligible (Hoorweg finds 1·5 per cent., and Garibaldi even 92 per cent; this last, it is true, at a lower temperature); and still less intelligible the constant reference to the absorption of the sun's rays by the action of aqueous vapour—a fact which Secchi*, for example, amongst others, supports. It is true that Tyndall employed aqueous vapour of much higher tension than could be present on Mont Blanc.

The weight of vapour present in the layer of air between the Glacier des Bossons and the summit of Mont Blanc may be calculated by means of a formula given by Hann†. The data required are the difference in height $h=3610$ metres, and the tension of the aqueous vapour p_0, in millimetres, on the Glacier des Bossons, which, according to Violle's measurements, was 5·3 millims.; also the temperatures of the upper and lower stations t_1 and t_2, which, according to Violle's data, were 1° C. and 9°·5 C.

$$Q=\frac{0{\cdot}0010582}{1+\alpha\frac{t_1+t_2}{2}}p_0\times 0{\cdot}2832\left\{1-10^{-\frac{h}{6517}}\right\}=11{\cdot}3 \text{ kgrm.}$$

Then 11 kgrm. would saturate a layer of air of 1040 cubic metres at a temperature of 12°, which was the temperature in Tyndall's laboratory. One metre of this layer would absorb 0·017 per cent. if the whole of the absorption were due to the aqueous vapour: this number must be multiplied by 6 to make it applicable to Tyndall's experiments; and then, from the equations

$$99{\cdot}90=100e^{-1{\cdot}x},$$
$$95\quad\ =100e^{-y{\cdot}x},$$

we find *that, taking Violle's measurements as correct, a 5-per-cent. absorption of heat-radiation from a body heated to* 270° *would require a length of at least* 50 *metres of saturated aqueous vapour at* 12° C., *whilst Tyndall (and, in a smaller degree, Hoorweg) finds the same absorption for a length of* 1·22 *metre.*

From what has been said it is clear that a definitive solution of the question of the actual absorption in our atmosphere cannot be given at present—the more so, since the carbonic acid and the vegetable and animal organisms in the air must be taken into account in deciding this question.

A decisive solution can only be obtained when the absorption is known for the different parts of the spectrum.

For the absorptions, so far as they have been measured, are

* Pogg. *Ann.* cxxvi.

† *Zeitschrift der österr. Ges. für Meteor.* Bd. ix.

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certainly made up of many portions, possibly compensating each other. On this account the results obtained in physical experiments are not applicable to the explanation of any molecular condition of matter whatever, and in future the investigation of absorption-spectra must be extended to the domain of heat-radiation.

These considerations induce us to attach but little value to the connexion between chemical composition and absorption which we have pointed out in this investigation.

Hence it follows that a comparison of the absorption-coefficients of different substances is only possible at equal pressures. Since, as already said, in no one of the cases examined was there any regular connexion between pressure and absorption, we chose a graphical method, in which we represented the pressures as abscissæ and the absorptions as ordinates. The absorption-coefficients of the substances examined, which belonged to the fatty series (containing nearly equal numbers of molecules) were, for the radiation of a source of heat at 100°, approximately in the following order:—

I. Methyl alcohol, formic acid, carbonic oxide, carbonic acid, chloroform.

II. Ethyl alcohol, acetic acid, ethyl ether, ethylene.

III. Butyl alcohol.

IV. Amyl alcohol.

Whilst the absorption of the substances placed in any one line appeared to us nearly the same (with exception of the probably not quite pure acetic acid), the absorption appears to increase rapidly with increasing percentage of carbon; and, further, the absorption appears to be essentially dependent on the number of carbon atoms directly combined with each other by simple linking. Thus, for example, benzol, in spite of its six carbon atoms, exerts only a very small absorption, probably in consequence of the mode in which the carbon atoms are combined.

Yet all these relationships may be purely accidental; not until we have a spectroscopic investigation of the ratios of absorption shall we be in a position to draw conclusions on the modes of vibration of the atoms. The extraordinary difficulty of investigations of this sort would be richly repaid by the attainment of quantitative results; whilst the corresponding optical investigations (immeasurably easier) will always remain more of a qualitative nature.

Physical Institute of Vienna.

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II. *A Relation between the Melting-points of the Elements and their Solid Binary Compounds and the Heats of Formation of the latter.* By THOMAS CARNELLEY, *D.Sc., and* L. T. O'SHEA, *Firth College, Sheffield**.

THE numerous determinations which have been made by Berthelot and Thomsen of the heat evolved in various chemical reactions have led us to compare the numbers so obtained for the solid halogen compounds of the elements with their melting-points and those of their constituent elements, as it appeared not improbable that some interesting relations might be detected between these physical constants.

In the present paper are detailed the results which have been obtained by such a comparison.

(1) In any chemical reaction such as

$$Ag_2 + Cl_2 = 2AgCl + \text{heat evolved},$$

the sum of the energies on one side of the equation is equal to the sum of the energies on the other side. The chemical energy of a body is measured by the inverse of the heat necessary to separate the atoms constituting the molecule—or, in the case where the molecule consists of but one atom, of unlocking in that atom the affinities which mutually saturate one another. Therefore the inverse of the heat necessary to separate the two atoms of silver + the inverse of that required to separate the two atoms of chlorine = twice the inverse of the heat required to separate one atom of silver from one atom of chlorine + the heat evolved.

(2) After many attempts to obtain a numerical measure of the chemical energy of a molecule of any substance, we finally came to the conclusion that it might be approximately represented by the melting-point of the body in such a way that the chemical energy of a molecule of any *solid* substance is inversely as its melting-point measured from the absolute zero (−273° C.).

Before proceeding further, we wish expressly to state that the inverse of the melting-point of a substance is taken merely as an *empirical* measure of its chemical energy, because as yet a true measure of it has not been discovered; and by using it in this way we have been led to the relations given below.

Assuming, then, tentatively that the inverse of the melting-

* Communicated by the Authors.

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point is such a measure, it will follow that in the equation

$$Ag_2 + Cl_2 = 2AgCl + \text{heat evolved},$$

the inverse of the melting-point of silver + the inverse of the melting-point of chlorine — twice the inverse of the melting-point of silver chloride will give a number proportional to the heat evolved. Let x be this number; then

$$\left.\begin{aligned} &Ag_2 + Cl_2 = 2AgCl + \text{heat evolved}, \\ &\frac{1}{1273} + \frac{1}{198} = \frac{2}{724} + x. \end{aligned}\right\} \quad . \quad . \quad (1)$$

Therefore

$$x = \frac{1}{1273} + \frac{1}{198} - \frac{2}{724}.$$

By carrying out calculations in this way for all the solid chlorides, bromides, and iodides of those solid elements (including K, Na, Li, Ag, Cu′, Mg, Zn, Cd, Ba, Pb, Sn″, Sn^{iv}, Cu″, Al, Tl, and Sb) of which the necessary melting-points and heats of formation have been determined, we obtain numbers for the heats of formation which follow the same, or nearly the same, order of size as the experimental heats of formation. The numbers, however, though following the same order of magnitude, are not *proportional* to those found by experiment. This is due to the fact that this relation strictly applies only when all the reacting and resulting bodies are *solid* at the ordinary temperature, to which the experimental heat-determinations are generally referred. For the iodides among themselves the relation is a quantitative one; but for the chlorides and bromides this is not the case, since chlorine is a gas and bromine a liquid at the ordinary temperature, so that their chemical energy in these physical conditions will not be represented by the inverse of their melting-points. They have, however, been calculated as follows from the experimental heats of formation of KI, AgBr, and AgCl, and the melting-points of K, Ag, I, KI, AgBr, and AgCl, thus :—

Let a = the value required for Cl_2,
b = „ „ Br_2.

Then from equation (1) we have

$$\frac{2K + I_2 - 2KI}{Ag_2 + Cl_2 - 2AgCl} = \frac{\text{heat of formation of KI}}{\text{heat of formation of AgCl}}, \quad . \quad (2)$$

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or

$$\frac{\frac{2}{335}+\frac{1}{383}-\frac{2}{907}}{\frac{1}{1273}+a-\frac{2}{724}}=\frac{85}{35};$$

from which

$$a=0{\cdot}00460.$$

Again,

$$\frac{2K+I_2-2KI}{Ag_2+Br_2-2AgBr}=\frac{\text{heat of formation of KI}}{\text{heat of formation of AgBr}},$$

or

$$\frac{\frac{2}{335}+\frac{1}{383}-\frac{2}{907}}{\frac{1}{1273}+b-\frac{2}{700}}=\frac{85}{26{\cdot}1};$$

from which

$$b=0{\cdot}00404.$$

Therefore the value for Cl_2 $=0{\cdot}00460$,

and for Br_2 $=0{\cdot}00404$;

the inverse of their melting-points being

$$Cl_2=\frac{1}{198}=0{\cdot}00505,$$

$$Br_2=\frac{1}{251}=0{\cdot}00398.$$

By the use of the two former numbers for Cl_2 and Br_2, and the inverse of its melting-point for I_2, the following table has been drawn up. In the fourth column of this table, containing the experimental heats of formation, (B) after a number signifies Berthelot, (T) Thomsen, (A) Andrews, (F & S) Favre and Silbermann. In all cases the heats of formation, both calculated and experimental, are for the combination of one atom of halogen.

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Equation.	$M = \frac{x \times 10000}{\text{no. of atoms of halogen}}$	Heat of formation. Calculated = M × 2·5.	Heat of formation. N = found.	$\frac{N}{M}$ = Constant.
$\left\{\begin{matrix} 2K + Cl_2 = 2KCl + \text{heat} \\ \frac{2}{335} + 00460 = \frac{2}{1007} + x \end{matrix}\right\}$	43·0	107	105·0 (B) 105·6 (T) 104·5 (A) 101·0 (F & S) 102·7	2·4 (105)
$\left\{\begin{matrix} 2Na + Cl_2 = 2NaCl + \text{heat} \\ \frac{2}{369} + 00460 = \frac{2}{1045} + x \end{matrix}\right\}$ ...	40·6	101	97·3 (B) 97·7 (T) 94·8 (F & S)	2·4 (97·3)
$\left\{\begin{matrix} 2K + Br_2 = 2KBr + \text{heat} \\ \frac{2}{335} + 00404 = \frac{2}{972} + x \end{matrix}\right\}$	39·8	99	100·4 (B) 99·0 (B) 95·3 (T) 90·2 (F & S) 89·2	2·5 (99)
$\left\{\begin{matrix} 2Na + Br_2 = 2NaBr + \text{heat} \\ \frac{2}{369} + 00404 = \frac{2}{981} + x \end{matrix}\right\}$ ...	37·1	93	90·6 (B) 90·4 (B) 85·7 (T)	2·4 (90·6)
$\left\{\begin{matrix} 2Li + Cl_2 = 2LiCl + \text{heat} \\ \frac{2}{453} + 00460 = \frac{2}{870} + x \end{matrix}\right\}$	33·6	84	93·8 (T)	2·7 (93·8)
$\left\{\begin{matrix} 2K + I_2 = 2KI + \text{heat} \\ \frac{2}{335} + \frac{1}{383} = \frac{2}{907} + x \end{matrix}\right\}$	31·9	80	85·2 (B) 85·4 (B) 80·1 (T) 77·3 (F & S) 76·3	2·5 (80·1)
$\left\{\begin{matrix} 2Na + I_2 = 2NaI + \text{heat} \\ \frac{2}{369} + \frac{1}{383} = \frac{2}{902} + x \end{matrix}\right\}$	29·1	73	74·2 (B) 74·1 (B) 69·1 (T)	2·5 (74)
$\left\{\begin{matrix} Tl_2 + Cl_2 = 2TlCl + \text{heat} \\ \frac{1}{563} + 00460 = \frac{2}{700} + x \end{matrix}\right\}$	17·5	44	48·6	2·7 (48·6)
$\left\{\begin{matrix} Tl_2 + Br_2 = 2TlBr + \text{heat} \\ \frac{1}{563} + 00404 = \frac{2}{731} + x \end{matrix}\right\}$	15·5	39	46·4	2·9 (46·4)
$\left\{\begin{matrix} Tl_2 + I_2 = Tl_2I_2 + \text{heat} \\ \frac{1}{563} + \frac{1}{383} = \frac{1}{712} + x \end{matrix}\right\}$	14·9	37	35·6	2·4 (35·6)
$\left\{\begin{matrix} Ag_2 + Cl_2 = 2AgCl + \text{heat} \\ \frac{1}{1273} + 00460 = \frac{2}{724} + x \end{matrix}\right\}$ ...	13·1	33	29·4 (B) 29·2 (B) 34·8 (F & S)	2·6 (34·8)
$\left\{\begin{matrix} Cu_2 + Cl_2 = 2CuCl + \text{heat} \\ \frac{1}{1364} + 00460 = \frac{2}{707} + x \end{matrix}\right\}$ ...	12·6	32	33·1 (B) 35·6 (B) 32·9 (B) 32·8 (T)	2·6 (33)
$\left\{\begin{matrix} Cu_2 + Br_2 = 2CuBr + \text{heat} \\ \frac{1}{1364} + 00404 = \frac{2}{782} + x \end{matrix}\right\}$ ...	11·1	28	30·0 (B) 29·0 (B)	2·6 (29)

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Table (*continued*).

Equation.	$M = \frac{x \times 10000}{\text{no. of atoms of halogen}}$.	Heat of formation.		$\frac{N}{M}$ = Constant.
		Calculated = M × 2·5.	N = found.	
$\left\{\begin{matrix} Ag_2 + Br_2 = 2AgBr + \text{heat} \\ \frac{1}{1273} + \cdot00404 = \frac{2}{700} + x \end{matrix}\right\}$ ...	9·8	25	27·7 (B) 26·7 (B) 25·6 (F & S)	2·6 (26·1)
$\left\{\begin{matrix} Cu_2 + I_2 = Cu_2I_2 + \text{heat} \\ \frac{1}{1364} + \frac{1}{383} = \frac{1}{874} + x \end{matrix}\right\}$ ...	11·0	27	21·9 (B) 21·7 (B)	2·0 (21·9)
$\left\{\begin{matrix} Ag_2 + I_2 = Ag_2I_2 + \text{heat} \\ \frac{1}{1273} + \frac{1}{383} = \frac{1}{800} + x \end{matrix}\right\}$ ...	10·7	27	19·2 (B) 19·7 (B) 18·6 (F & S) 15·9	1·9 (19·7)
$\left\{\begin{matrix} 2Mg + 2Cl_2 = Mg_2Cl_4 + \text{heat} \\ \frac{2}{706} + (2 \times \cdot00460) = \frac{1}{981} + x \end{matrix}\right\}$	27·5	69	75·5 (B) 75·5 (T)	2·7 (75·5)
$\left\{\begin{matrix} 2Mg + 2Br_2 = Mg_2Br_4 + \text{heat} \\ \frac{2}{706} + (2 \times \cdot00404) = \frac{1}{971} + x \end{matrix}\right\}$	24·7	62	70·0 (B)	2·8 (70·0)
$\left\{\begin{matrix} 2Ba + 2Br_2 = Ba_2Br_4 + \text{heat} \\ \frac{2}{723} + (2 \times \cdot00404) = \frac{1}{1085} + x \end{matrix}\right\}$	24·8	62	84·7 (T)	3·4 (84·7)
$\left\{\begin{matrix} Cd_2 + 2Cl_2 = 2CdCl_2 + \text{heat} \\ \frac{1}{593} + (2 \times \cdot00460) = \frac{2}{814} + x \end{matrix}\right\}$	21·1	53	46·6 (B) 46·6 (T)	2·2 (46·6)
$\left\{\begin{matrix} Cd_2 + 2Br_2 = 2CdBr_2 + \text{heat} \\ \frac{1}{593} + (2 \times \cdot00404) = \frac{2}{844} + x \end{matrix}\right\}$	18·5	46	42·1 (B) 41·7 (B)	2·3 (41·9)
$\left\{\begin{matrix} Zn_2 + 2Cl_2 = 2ZnCl_2 + \text{heat} \\ \frac{1}{633} + (2 \times \cdot00460) = \frac{2}{535} + x \end{matrix}\right\}$	17·6	44	48·6 (B) 50·6 (A) 50·3 (F & S)	2·8 (48·6)
$\left\{\begin{matrix} Zn_2 + 2Br_2 = 2ZnBr_2 + \text{heat} \\ \frac{1}{633} + (2 \times \cdot00404) = \frac{2}{667} + x \end{matrix}\right\}$	16·7	42	43·6 (B) 43·1 (B) 40·6 (A)	2·6 (43·1)
$\left\{\begin{matrix} Zn_2 + 2I_2 = 2ZnI_2 + \text{heat} \\ \frac{1}{633} + \frac{2}{383} = \frac{2}{719} + x \end{matrix}\right\}$	10·1	25	29·6 (B) 30·0 (B) 26·6 (A)	2·8 (28·7)
$\left\{\begin{matrix} Cd_2 + 2I_2 = 2CdI_2 + \text{heat} \\ \frac{1}{593} + \frac{2}{383} = \frac{2}{677} + x \end{matrix}\right\}$	9·9	25	27·9 (B) 27·5 (B)	2·8 (27·7
$\left\{\begin{matrix} Pb_2 + 2Cl_2 = 2PbCl_2 + \text{heat} \\ \frac{1}{605} + (2 \times \cdot00460) = \frac{2}{771} + x \end{matrix}\right\}$	20·6	51	41·4 (B) 42·6 (B) 41·4 (T) 44·7 (F & S)	2·2 (44·7)
$\left\{\begin{matrix} Sn_2 + 2Cl_2 = 2SnCl_2 + \text{heat} \\ \frac{1}{503} + (2 \times \cdot00460) = \frac{2}{523} + x \end{matrix}\right\}$	18·4	46	40·4 (B) 40·4 (T) 40·2	2·2 (40·4)

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Table (*continued*).

Equation.	$M = \frac{x \times 10000}{\text{no. of atoms of halogen}}$	Heat of formation. Calculated $= M \times 2{\cdot}5$.	Heat of formation. N = found.	$\frac{N}{M}$ = Constant.
$Pb_2 + 2Br_2 = 2PbBr_2 + \text{heat}$ $\frac{1}{605} + (2 \times {\cdot}00404) = \frac{2}{772} + x$	17·8	44	36·3 (B) 36·2 (B) 38·5 (B) 32·8 (F & S)	2·2 (38·5)
$Sn_2 + 2Br_2 = 2SnBr_2 + \text{heat}$ $\frac{1}{503} + (2 \times {\cdot}00404) = \frac{2}{532} + x$	15·8	39	35·7 (B) 34·4 (B)	2·3 (35·7)
$Cu_2 + 2Cl_2 = 2CuCl_2 + \text{heat}$ $\frac{1}{1364} + (2 \times {\cdot}00460) = \frac{2}{771} + x$	18·4	46	32·9 (B) 31·0 (B) 25·8 (T) 30·5 (A) 29·5 (F & S)	1·8 (32·9)
$Sn_2 + 2I_2 = 2SnI_2 + \text{heat}$ $\frac{1}{503} + \frac{2}{383} = \frac{2}{589} + x$	9·5	24	27·0 (B)	2·8 (27·0)
$Pb_2 + 2I_2 = 2PbI_2 + \text{heat}$ $\frac{1}{605} + \frac{2}{383} = \frac{2}{656} + x$	9·6	24	25·2 (B) 26·4 (B) 23·2 (F & S) 26·9	2·6 (25·2)
$Sn_2 + 4Br_2 = 2SnBr_4 + \text{heat}$ $\frac{1}{503} + (4 \times {\cdot}00404) = \frac{2}{303} + x$	14·4	36	28·8 (B)	2·0 (28·8)
$Sn_2 + 4I_2 = 2SnI_4 + \text{heat}$ $\frac{1}{503} + \frac{4}{383} = \frac{2}{419} + x$	9·5	24	20·0 (B)	2·1 (20·0)
$Al_2 + 3Cl_2 = Al_2Cl_6 + \text{heat}$ $\frac{1}{873} + (3 \times {\cdot}00460) = \frac{1}{453} + x$	21·2	53	53·6 (B) 53·6 (T)	2·5 (53·6)
$Al_2 + 3Br_2 = Al_2Br_6 + \text{heat}$ $\frac{1}{873} + (3 \times {\cdot}00404) = \frac{1}{366} + x$	17·6	44	44·2 (B)	2·5 (44·2)
$Sb_4 + 6Cl_2 = 4SbCl_3 + \text{heat}$ $\frac{1}{710} + (6 \times {\cdot}00460) = \frac{4}{345} + x$	14·5	36	30·4 (B) 30·5 (A) 28·8	2·1 (30·5)
$Al_2 + 3I_2 = Al_2I_6 + \text{heat}$ $\frac{1}{873} + \frac{3}{383} = \frac{1}{398} + x$	10·8	27	23·4 (B)	2·2 (23·4)
$P_4 + 6I_2 = 4PI_3 + \text{heat}$ $\frac{1}{317} + \frac{6}{383} = \frac{4}{328} + x$	5·5	14	3·5 (B)	0·6 (3·5)

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From the above table it is seen that, by dividing the experimental heat of formation (N) by the number in the second column (M), a nearly constant number is obtained, which is given in the last column. The mean of the numbers for $\frac{N}{M}$ = 2·46 or about 2·5, which latter has been assumed as correct. On multiplying M by this number 2·5, we obtain the calculated heats of formation as shown in the third column; and these agree fairly well with the experimental heats of formation in the fourth column. The bracketed numbers in the last column represent those values of N which have been used in finding the constant $\frac{N}{M}$.

In the following table are given the percentage differences between the calculated and experimental heats of formation:—first (column I.), as referred to the experimental numbers given in brackets in the last column of the preceding table; and second (column II.), to the nearest extreme experimental number, when the calculated number is either greater or less than *all* the experimental numbers. For comparison, there is given in column III. the percentage difference between the greatest and least of the experimental values reckoned on the bracketed numbers in the last column of the preceding table.

	I.	II.	III.		I.	II.	III.
KCl	1·9	1·3	4·4	$CdCl_2$	14·0	14·0	0
NaCl	3·0	3·0	3·0	$CdBr_2$	9·0	9·0	—
KBr	2·0	0	12·0	$ZnCl_2$	9·0	9·0	4·0
NaBr	2·2	2·2	5·4	$ZnBr_2$	2·5	0	7·0
LiCl	10·0	10·0	—	ZnI_2	13·0	5·5	11·0
KI	6·0	0	11·0	CdI_2	9·0	9·0	—
NaI	1·3	0	7·0	$PbCl_2$	14·0	14·0	7·3
TlCl	9·0	9·0	—	$SnCl_2$	14·0	14·0	0·5
TlBr	16·0	16·0	—	$PbBr_2$	14·0	14·0	15·0
TlI	4·0	4·0	—	$SnBr_2$	9·0	9·0	—
AgCl	6·0	0	16·0	$CuCl_2$	39·0	39·0	22·0
CuCl	3·0	2·4	8·5	SnI_2	11·0	11·0	—
CuBr	3·3	3·3	3·3	PbI_2	4·0	0	15·0
AgBr	4·0	2·3	8·0	$SnBr_4$	24·0	24·0	—
CuI	25·0	25·0	—	SnI_4	20·0	20·0	—
AgI	37·0	37·0	19·0	Al_2Cl_6	1·1	1·1	0
$MgCl_2$	8·0	8·0	0	Al_2Br_6	0	0	—
$MgBr_2$	11·0	11·0	—	$SbCl_3$	18·0	18·0	5·7
$BaBr_2$	27·0	27·0	—	Al_2I_6	15·0	15·0	—
				PI_3	300·0	—	—
			Mean of all except PI_3 =		11·0	10·0	8·0

The average difference between the heats of formation as found by experiment and by calculation from the melting-

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points is 11 per cent., whilst the average difference between the heats of formation as found experimentally for the same compound by different observers is 8 per cent.; which shows that the method of calculating the heats of formation from the melting-points gives results as near as could be expected, and especially when we take into consideration:—

(1) That no less than three separate melting-points are employed in each calculation, thus rendering the latter liable to three sources or error.

(2) That the size of the molecules in the solid state, both of the elements entering into the reaction and of the compounds produced, are in most cases quite unknown.

(3) That many of the melting-points employed are subject to considerable uncertainty; thus those for Ba vary between 450° C. and the melting-point of cast iron, those for Cu between 950° and 1330°, those for Ag between 916° and 1040°, for Mg between 433° and 750°, those for Al between 600° and above 890°, for Cd between 228° and 325°, for Zn between 360° and 450°, for Pb between 320° and 335°, for Sn between 220° and 246°, and for Sb between 425° to 621°.

(4) That the high average difference between the calculated and experimental heats of formation is chiefly due to large errors in a comparatively few cases; for only about one third (14 out of 38) of the whole number of instances give differences *above* the average, and in 6 out of the 14 only *one* experimental determination of the heat of formation has been made, whilst in four of the remaining 8 the difference between the experimental numbers themselves is very great. Further, in 8 of the 14 instances in which differences above the average occur, the determinations of the melting-points of the metals differ by as much as 100° to 400° C., and in the case of Ba even about 1200°.

On the other hand, in some cases it is only by adopting molecules of a size different from that usually ackowledged that numbers agreeing with our theory are obtained. Also in calculating the ratio $\frac{N}{M}$, we have in a few instances used that heat of formation most favourable to the theory.

In the tables we have given all the examples to which our method of calculation can be applied; but whether the above relation does exist or not requires confirmation in other cases, for which, however, the necessary data are at present wanting.

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III. *On Action at a Distance.*
By Oliver J. Lodge, *D.Sc.**

THE idea of immediate action at a distance has been so thoroughly condemned by Newton, Clerk Maxwell, and other leaders of science in this country, that one cannot but rejoice that Mr. Walter Browne has thought it worth while to undertake its defence and to plead whatever can be pleaded on its behalf, because there is a danger that in discarding too completely any point of view some valuable truth may be temporarily lost sight of at the same time.

In his carefully constructed argument, communicated by the Physical Society of London to the December number of the Philosophical Magazine, Mr. Browne has mainly occupied himself with certain views concerning the æther, more especially with the one whose propoundment at the present time is a natural consequence of the striking success of the kinetic theory of gases, the one, namely, which regards every phenomenon in the universe as due to the bombardment of freely flying atoms of various sizes and length of path.

I am not anxious to defend this interesting hypothesis, which, whether it ultimately maintain its ground or not, is sure to find supporters; and I do not see that it very materially affects the question as to whether there is or is not such a thing as direct action of one body or another across a distance—because, as Mr. Browne and others have observed, the magnitude of the distance across which the action takes place is of no moment whatever. The real point in Mr. Browne's argument in favour of action at a distance consists, I think, in the metaphysical difficulties which he finds in assuming the existence of immediate contact between two bodies under any circumstances.

But I venture to think that, putting metaphysics entirely on one side, we may prove in a perfectly simple and physical manner that it is impossible for two bodies *not* in contact to act directly on each other; in other words, we can show that action at a distance is incompatible either with the law of the conservation of energy, or with Newton's "third law," or with both.

Let us state these two axioms, with the necessary definitions, thus:—

1st. If a body A act directly and solely on a body B, the force with which A acts on B is equal and opposite to the force with which B acts on A.

2nd. If B yield to the force exerted by A, A is said to do

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"work," and B to have work done upon it—the work done in each case being measured by the product of the force exerted into the distance moved through in the direction of the force; and A is said to have lost an amount of "energy" numerically equal to the work done by it, and B is said to have gained an amount of energy equal to the work done upon it. But there is no loss or gain of energy on the whole; so the energy is simply transferred from A to B by the act of work; or the work done by B upon A is equal and opposite to the work done by A upon B.

Now, if these premises are granted, it follows at once, since the forces (F) are equal and opposite, and the works (Fs) are also equal and opposite, that the distances (s) must be equal but not opposite; that is, that the two bodies must move over precisely the same distance and in the same sense; which practically asserts that they move together and are in contact, so long as the action is going on.

All this is utterly at variance with what happens when the earth pulls a stone, or when a magnet attracts a piece of iron, or when a gun fires a bullet. The distances moved through by the two bodies are by no means the same, and are even in opposite senses; so that, so far from energy being transferred from one body to the other, some energy is gained by both bodies, though very little by the larger one.

Hence, admitting the premises, we are bound (and of course perfectly willing) to conclude that the gun does not fire the bullet, but that the powder fires both the bullet and the gun: the gun, in fact, has no direct action on the bullet at all, it does not touch it; it is the powder which is in contact with each, does work on each, and communicates some of its energy to each.

But we are equally bound to conclude that the earth has no direct action on the stone, nor the magnet on the iron, but that the medium surrounding both presses them together: it is in contact with each, does work on each, and communicates some of its energy to each. The nature of this medium and of its contact happen to be both utterly unknown; but otherwise the three cases are analogous.

I could illustrate my meaning more fully by examining these examples in greater detail or by taking others; but I believe it to be unnecessary. I may, however, be permitted to refer Mr. Browne to a short paper on Energy which appeared in the Philosophical Magazine for October 1879.

University College, London.

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IV. *On Action at a Distance.* *By* S. Tolver Preston*.

IN the last Number of the Philosophical Magazine is a paper by Mr. Walter R. Browne, "On Action at a Distance"†, in which some conclusions of mine relating to the explanation of gravitation, or the general phenomena of approach, under the kinetic theory of the æther, are noticed.

Although some of the objections cited by Mr. Browne do not appear to me to be of much weight, and some will, I think, be found to be answered in my previous papers‡, I will nevertheless comment upon a few points here.

First, on page 440, a difficulty originally brought forward by Dr. Croll§, in a notice of my papers, is alluded to, relating to the porosity of matter required by the kinetic theory of gravity. It should be observed, however, that this difficulty was considered by me (Philosophical Magazine, Feb. 1878), and a means suggested for surmounting it‖.

Secondly, Mr. Browne concludes, apparently rather gratuitously, on page 441 as follows, viz.:—"Hence our conception of the gravity-gas must practically be that of an indefinite number of indefinitely small particles moving in all directions with indefinitely high velocities—a conception from which it hardly seems safe to draw any definite conclusion whatever."

It is scarcely necessary to add that the number of particles in unit of volume, their dimensions, and mean¶ velocity, are not actually assumed by me to be indefinite (but strictly finite). Any finite values, however great, are still an unlimited distance from *indefinite* values.

Thirdly, on page 442, the fact is seemingly overlooked that, according to the vortex-atom theory (alluded to in the context), it appears that molecules would be elastic**; so that it would not seem necessary to assume that the molecules of a solid bar are normally *at a distance* from each other in order to explain

* Communicated by the Author.

† Read before the Physical Society, November 13, 1880.

‡ Philosophical Magazine, Sept. and Nov. 1877, Feb. 1878.

§ Ibid. Jan. 1878.

‖ It is obvious that if the *molecules of matter themselves* have an open structure, then matter may possess any degree of porosity (or permeability to the particles of the gravific æther) that observed facts may require. The open structure of molecules is only *à priori* natural, since a solid (block) structure of molecules would involve useless waste of material.

¶ The high velocity of the particles is the perfectly natural consequence of their minute mass.

** It is sufficiently clear that if "elasticity" were not yet explained, the first step towards this end would have to be the rejection of "action at a distance," since the retention of an occult quality would render any *explanation* hopeless.

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the contraction of the bar (within certain limits) under an applied pressure—the bar being elastic partly through the elasticity of the open structure of its molecules*.

Fourthly, it appears to be assumed by Mr. Browne that the postulate of "action at a distance" affords an alternative *explanation* of facts, as on page 444 it is remarked as follows, viz.:— . . . . "it must be held to be demonstrated that the phenomena of cohesion cannot be *explained*† except on the hypothesis of action at a distance."

It may, surely, well be asked here how that which is in itself inexplicable can *explain* any thing, or how the assumption of an occult quality can throw light upon any problem whatever.

In conclusion, it cannot fail, I think, to be apparent (as an important fact) to an impartial observer, that a movement in accordance with the *kinetic* theory is the only possible (or conceivable) motion that can naturally maintain itself among particles of matter left to themselves in free space (if we refrain from attributing to matter occult and mystical qualities, which only involve every thing in obscurity).

The application of the *kinetic* theory to the phenomena of sound, light‡, gravity, and (possibly) to the motions of the larger-scale stellar masses of the universe§ immersed in the kinetic æther (as developed by me in former essays in this Journal and elsewhere) would seem to afford some hope of ultimately correlating a wide range of phenomena under one fundamental cause of extreme simplicity.

* To afford some rough idea of the mode in which molecules of open structure may be conceived to be held together (in cohesion) by the pressure of streams of particles, I would refer to my paper entitled "A Suggestion in regard to Crystallization," Phil. Mag. April 1880.

† The italics are mine.

‡ 'Nature,' Jan. 15, 1880, "On a Mode of Explaining the Transverse Vibrations of Light."

§ Philosophical Magazine, August 1879 and November 1880.

The difficulty of the explanation of magnetism (alluded to by Mr. Browne on page 444) may be freely admitted. But magnetism is a somewhat special phenomenon, dependent possibly on special (or secondary) conditions. It would surely be scarcely reasonable to expect that the theory should be capable of giving full satisfaction in all cases. It would be even strange if some difficulties did not present themselves at the outset. We can only say that by explaining some important or fundamental facts (such as gravitation, some effects of cohesion, &c.), the hope may be reasonably entertained that an addition to knowledge will throw light upon others—so long as we hold to strict mechanical conceptions, and do not close the door to discovery by postulating the mystery of "action at a distance." (See also paper "On Method in Causal Research," Phil. Mag. May 1880, in connexion with this.)

Among all the arguments expended on "action at a distance," it certainly appears strange that the one firm and indisputable fact is not more

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V. *Implicational and Equational Logic.*

By HUGH MCCOLL, *B.A., University of London**.

PROF. JEVONS, in his new work, 'Studies in Deductive Logic,' of which he has kindly sent me a copy, refers to my papers in 'Mind' and in the 'Proceedings of the London Mathematical Society' in terms which might give rise to some misapprehension as to the real nature of my symbolical method. He says that "I reject equations in favour of *implications*," and in so doing "ignore the necessity of the equation for the application of the Principle of Substitution."

Now, it is quite true that I reject equations in favour of implications in those classes of logical problems (and they are very numerous) in which implications lead to the simplest, shortest, and most elegant solutions; but there are other classes of problems, especially in mathematics, which necessitate the equational form of statement; and in these I do not hesitate to adopt it. The simple truth is, that my method admits of both forms; and, as a matter of fact, I employ both, sometimes even in the same problem. In my first paper in the Proceedings of the London Mathematical Society (which treats of the limits of multiple integrals) I adopt the equational form throughout; in my second and third papers, which relate entirely to questions of pure logic, I generally adopt the implicational form, as the simplest and most effective; while in my fourth paper, which treats of probability, I mainly adopt the equational form.

As to the statement that "I ignore the necessity of the equation in the application of the Principle of Substitution," I am not quite sure that I understand what it means. I certainly recognize the principle that if $\alpha=\beta$, then $f(\alpha)=f(\beta)$, or, as the rule may be expressed symbolically in my notation, $(\alpha=\beta):\{f(\alpha)=f(\beta)\}$; but I cannot in the least understand what bearing this has upon the advantages or disadvantages of my system of implications.

The question whether the implication $\alpha:\beta$, or its equivalent, the equation $\alpha=\alpha\beta$, should be preferred in a symbolical system of logic, must be decided on the broad grounds of practical convenience. I believe it may be taken as a useful

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clearly kept in view, viz. that this assumption or theory, by opening out an absolutely limitless field of speculative hypothesis, completely annihilates all *method* or rational system in physical inquiry, and therefore that all progress or insight into the physical processes underlying phenomena is absolutely brought to a standstill so long as this theory is adhered to.

* Communicated by the Author.

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principle in symbolical reasoning generally, that conventional symbols of abbreviation should be adopted for all expressions *which have to be employed frequently*. On this principle a^3 was probably used as a convenient abbreviation for aaa, a^4 for $aaaa$, and so on, before the discovery of the important law expressed by the equation $a^m \times a^n = a^{m+n}$. The same necessity for symbolical abbreviation originated the useful symbols $f(x)$, $f(x, y)$, $f'(x)$, and many others. On this principle, since I find that such statements as "If α is true β is true," or "α implies β," *are extremely common in all reasoning*, I use the simple symbol $\alpha : \beta$ as a very convenient abbreviation*. Granted that the equation $\alpha = \alpha\beta$ will also accurately express the statement "α implies β," it is a much less simple and suggestive expression for it. Compare, again, the implication, $\alpha\beta + \gamma\delta : ab + cd$, with its equivalent, the equation $\alpha\beta + \gamma\delta = (\alpha\beta + \gamma\delta)(ab + cd)$, and the superior simplicity of the implication will be still more striking. But the abbreviating power of my symbol of implication becomes most conspicuous in what may be called *implications of the second order*, as in the syllogism

$$(\alpha : \beta)(\beta : \gamma) : (\alpha : \gamma).$$

May I ask Prof. Jevons how he would express this syllogism in his equational notation, *in pure symbols and entirely without words*. I can only see one way in which he could do this consistently with his views, namely by the very clumsy equation

$$(\alpha = \alpha\beta)(\beta = \beta\gamma) = (\alpha = \alpha\beta)(\beta = \beta\gamma)(\alpha = \alpha\gamma).$$

This looks so exceedingly like a *reductio ad absurdum*, that I cannot help hoping that it will lead Prof. Jevons to reconsider his opinion that *the equational form alone* should be employed in symbolical logic.

So far I have argued on the assumption that my $\alpha : \beta$ is equivalent to Prof. Jevons's $\alpha = \alpha\beta$; and both Prof. Jevons and I agree, I believe, in the opinion that practically this is the case. At the same time, it must be borne in mind that, for this assumption to be strictly true, the letters α and β must have the same meanings in the implication $\alpha : \beta$ as in the equation $\alpha = \alpha\beta$; and therefore either each letter must in both forms

* The equivalence of $\alpha : \beta$ and $\alpha = \alpha\beta$ may be proved formally in my notation as follows:—

From the formula

$$(\alpha = \beta) = (\alpha : \beta)(\beta : \alpha)$$

we get

$$(\alpha = \alpha\beta) = (\alpha : \alpha\beta)(\alpha\beta : \alpha) = (\alpha : \alpha)(\alpha : \beta)(\alpha\beta : \alpha) = (\alpha : \beta);$$

for the factors $\alpha : \alpha$ and $\alpha\beta : \alpha$ are each equal to unity—that is to say, *always true*, whatever the statements α and β may be. (See formula 3 of this paper further on.)

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denote a *statement*, as in my system, or else each letter must in both forms denote a *quality or thing*, as in Prof. Jevons's system. On the supposition that each letter denotes a statement, my notation exhibits clearly the very remarkable fact that in the syllogism $(\alpha : \beta)(\beta : \gamma) : (\alpha : \gamma)$, the very same relation which connects α with β, and β with γ, connects also the combined premises $(\alpha : \beta)(\beta : \gamma)$ with the conclusion $\alpha : \gamma$. On the assumption that each letter denotes a statement, Prof. Jevons's notation (as I have shown) could only show this coincidence of relation in a very clumsy and roundabout manner; while, on the assumption that each letter denotes a thing or quality (as in his system), his notation could scarcely be used in this extended way at all.

The same remarks apply to many other useful and symmetrical formulæ, which, so far as I can see, are altogether uninterpretable on Prof. Jevons's hypothesis that each letter should denote a thing or quality; while on my hypothesis, that each letter should denote a statement, every formula conveys a clear and precise meaning, which it is scarcely possible to misunderstand. Take, for example, the formulæ:—

(1) $(A : a)(B : b)(C : c)\ldots : (ABC\ldots : abc\ldots)$;

(2) $(A : a)(B : b)(C : c)\ldots : (A+B+C+\ldots : a+b+c+\ldots)$;

(3) $(x : a)(x : b)(x : c)\ldots = (x : abc\ldots)$;

(4) $(a : x)(b : x)(c : x)\ldots = (a+b+c+\ldots : x)$;

(5) $(a : x)+(b : x)+(c : x)+\ldots : (abc\ldots : x)$,

(6) $(x : a)+(x : b)+(x : c)+\ldots : (x : a+b+c+\ldots)$.

These formulæ express logical laws of undoubted truth, which Prof. Jevons could scarcely express in his notation without the help of words.

Prof. Jevons approves to some extent of my accent to express denial, and occasionally adopts this notation in his new work; but he finds it difficult, he says, to believe that there is any advantage in my innovations in other respects, and he is of opinion that "my proposals tend towards throwing Formal Logic back into its ante-Boolian confusion." To this general condemnatory opinion it is difficult to make any definite reply; I can only express my regret that Prof. Jevons has nowhere throughout his book given a single example of this tendency in my proposals "towards throwing Formal Logic back into its ante-Boolian confusion." Abundant materials were at his disposal for comparing my method with his own in the fairest and most decisive way possible, namely in the actual solution of problems. Out of the various problems of which I have published solutions he might surely have found *one* with which

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to point and illustrate his criticism. Friendly contests are at present being waged in the 'Educational Times' among the supporters of rival logical methods; I hope Prof. Jevons will not take it amiss if I venture to invite him to enter the lists with me, and there make good the charge of "ante-Boolian confusion" which he brings against my method.

November 29, 1880.

VI. *Note on* Prof. Exner's *Papers on Contact Electricity.* *By* W. E. Ayrton *and* John Perry*.

I. IN the autumn of 1879 Prof. Fleeming Jenkin drew our attention to a paper by Prof. Exner, read before the Vienna Academy of Science, and appearing in the July number of their 'Transactions' for that year. This paper will also be found reprinted in this year's April number of Wiedemann's *Annalen der Physik und Chemie*; and quite recently an English translation, prepared by Mr. Brown, has appeared, in the October number of the Philosophical Magazine.

As then, this paper has been deemed of sufficient importance to be printed at least three times; and as the reasoning employed in it is of so plausible a nature as to mislead a casual reader, and to give him erroneous notions on the subject of contact electricity, we have thought it worth while to draw attention to the inaccuracies it contains.

The calculation given in the Phil. Mag. for 1851 by Sir Wm. Thomson of the electromotive force of a Daniell's cell, based on the principle of the conservation of energy, is of course well known. The method employed, which was due to Dr. Joule, is as follows:—The work done by a quantity of electricity Q passing between two points at a difference of potential E is EQ. Now if this electromotive force is produced by a Daniell's cell, the preceding quantity of work must be equal to the energy-equivalent of the chemical changes that take place in this cell when a quantity of electricity Q passes through it. And since this latter can be determined from the heats of combustion of the products decomposed and formed, and from a knowledge of Joule's mechanical equivalent of heat, Sir Wm. Thomson was enabled to calculate the electromotive force of the Daniell's cell from the supposed known chemical reactions taking place in it. It has been pointed out by Dr. Wright†, and by others, that the great coincidence between the electromotive force of the

* Communicated by the Physical Society, having been read at the Meeting on November 13, 1880.

† "On the Determination of Chemical Affinity in Terms of Electromotive Force," by C. R. Alder Wright, D.Sc., Phil. Mag. April 1880, p. 247.

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Daniell's cell, thus calculated, and the value obtained by a direct measurement with an absolute electrometer would not have been arrived at by Sir William had he had at his disposal the more accurate determinations that have since been made of the heats of combustion. This, however, as we think may be gathered from Dr. Wright's paper, does not in the least disprove the accuracy or detract from the utility of Sir Wm. Thomson's, or rather Joule's, theory, but merely shows that the products of combustion in the Daniell's cell are not exactly what have usually been supposed; for it has to be remembered that if energy is developed or absorbed by occlusion of gases, or by any other physical action that has not hitherto been included in chemical actions, this must all be taken into account in the calculation of electromotive force.

II. Now Prof. Exner expresses the opinion that the so-called contact electricity is produced by the oxidation of the metals in contact with the oxygen of the air, in the same way as electricity is evolved by the oxidation of the zinc in galvanic cells. He says he has proved that this is true, and states, in consequence, that the electromotive force between two metals in contact with the air must be measured and expressed by the heats of combustion. The exact relationship which, in his opinion, ought to exist between the contact electromotive force of, say, zinc and copper and the difference of the heats of combustion of equivalent quantities of these metals is the fundamental part of his paper.

He says, at the bottom of page 598, Heft 4, Band ix. Wiedemann's *Annalen*, 1880, as we have translated it:—"We know that in cells each chemical process produces a potential-difference which is proportional to its heat-value; and in the case of the oxidation of a metal in air, we should expect that the potential-difference between a metal and the oxide produced would be proportional to the heat of combustion. Therefore any metal which, when insulated in the air, oxidizes, must contain a certain quantity of separated positive and negative electricity; and it is obvious that these charges are inactive towards outside bodies. These separated electricities cannot surpass a certain tension-difference; for the observed tension is always a constant, no matter if the oxidation continues or not. From this it would appear that the quantities of electricity produced in excess through continual oxidation are neutralized again with the production of the corresponding quantity of heat.

"If now, for example, a piece of zinc through oxidizing in the air has a potential $+E$, the oxidized layer, or rather the layer of air which is in contact with it, has, on the contrary,

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the potential $-E$; so that the potential-difference is equal to 2E, which must be measured by the heat of combustion of the zinc. If we now unite the zinc with any indifferent metal, for example platinum, then a part of the electricity will flow over to the platinum until both the metals have a common potential $+P$. The free tension on the zinc becomes now $-E+P$, that of the platinum $+P$; therefore the potential-difference between zinc and platinum is now $-E$, which is measured by half the heat of combustion of the zinc. If, therefore, the heat-value of the Daniell's cell be A (the heat-values, as is well known, must have reference to the chemical equivalents of the substances), and the heat of combustion of the zinc be B, then the potential-difference between zinc and platinum would be equal to $\frac{B}{2A}$. If the metal connected with the zinc is oxidized in the air, the manner of considering the subject still remains the same, or the potential-difference between the metals is measured by half the difference of the heats of combustion."

It will be observed that Prof. Exner speaks of the difference of potentials between a metal and its oxide as determinable from the heat of combustion; we presume, then, this difference must be a constant, what in fact he calls 2E. Now let us consider the second part of his statement given above; and we find that by joining platinum metallically to zinc the platinum and zinc have a common potential which he calls $+P$; yet the difference of potential between the platinum and the oxide of zinc is now only $+E$, or half the previous difference between zinc and its oxide. Although, then, he commences by saying that the difference of potential between zinc and its oxide is measured by the heat of combustion, which we know, per chemical equivalent, is a constant, he ends by concluding that the difference of potential is *not* a constant.

But it may be suggested that as his language is a little vague, and not in accordance with that employed in the modern mathematical theory of electricity, perhaps he means that there is a certain charge of electricity in the oxide which is a constant. This, however, does not appear to be his meaning, since a very simple consideration will show that if the charge of electricity in the oxide were a constant, the difference of potentials between zinc and platinum in metallic contact, as measured by the Volta experiments, would not be, as experiment shows, fairly constant for clean dry zinc and platinum (viz. 0·981 volt), but would be a variable depending on the condensing-arrangement employed.

[*Addition*, Dec. 21st.—This latter argument, Mr. Brown

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says, in the 'Electrician' for Dec. 4th, he does not follow. If, however, he will refer to Prof. Exner's own description of the measuring-apparatus employed, he will, by applying the ordinary mathematical laws, see that the method of attaching a Daniell's cell to the zinc and platinum plates in Volta's condenser, first with its poles one way and then reversed, measures, in terms of the Daniell's cell, the difference of potential between a point in the air close to the zinc plate and a point close to the platinum; but cannot possibly give any measurement of the actual electric charge in the zinc oxide or in Mr. Brown's "condensed compound gas-film," or in the platinum plate itself, unless we know the actual distance between the plates forming the condenser.]

III. We are justified, then, in concluding that he means that there is neither a constant difference of potentials between zinc and its oxide, nor a constant quantity of separated electricity. What, then, is his assumption? As far as an attentive study of his paper on our part can lead us, it is simply this:—The potential-difference between two metals in contact in air is measured by *half* the difference of the heats of combustion—an assumption which, in spite of the semblance of reasoning employed in the previous sentences, our knowledge of electricity gives us no basis for making. But Prof. Exner's sets of experiments I., II., III., consisting of direct measurements with Kohlrausch's apparatus of the electromotive forces of contact of zinc and platinum, copper and platinum, and iron and platinum, combined with J. Thomsen's determinations of the heats of combustion, appear to lend a most powerful support to Prof. Exner's assumption. Are we not justified, then, in accepting it as a conclusion proved by experiment, although it could not have been previously arrived at from our existing knowledge of electricity? Now it is unfortunate for this conclusion that Prof. Hoorweg, in the *Annalen der Physik und Chemie*, No. 9, Band xi. Heft I. pp. 133–155, 1880, has taken exception to Prof. Exner's experiments themselves; and, to make this plain, he gives the following table:—

		Kohlrausch*.	Hankel.	Exner.	Ayrton and Perry (1880).
Zn	Pt . .	0·984	0·984	0·881	0·981
Cu	Pt . .	0·184	0·184	0·367	0·238
Fe	Pt . .	0·384	0·312	0·704	0·369

* Prof. Exner says that since the time of Kohlrausch "only isolated and untrustworthy contact experiments have been made." As, however, those even carried out by Kohlrausch himself with metals appear to disagree with those of Prof. Exner, while they agree with the results obtained by other experimenters, Prof. Exner might consistently have included Kohlrausch's name in his sweeping condemnation.

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Our results as given above by Prof. Hoorweg are in terms of a volt, and not in terms of the electromotive force of a Daniell's cell, which is the standard employed by the previous observers. To reduce our numbers to the same standard, they must be diminished by about 10 per cent. They become then :—

Ayrton and Perry.

Zn	Pt	. .	0·883	of a Daniell's cell.
Cu	Pt	. .	0·214	
Fe	Pt	. .	0·332	

From these numbers it will be seen that while Kohlrausch, Hankel, and ourselves, although making the experiments in quite different ways, and with necessarily different specimens of the metals, have obtained fairly consistent results, those of Prof. Exner, with the exception of the first, stand by themselves. Nevertheless all his results agree with surprising accuracy with the numbers required by his theory.

IV. On page 597 of his paper, Prof. Exner refers to the early experiments of Pfaff, which proved that the measured electromotive force of contact of two metals was independent of the gaseous medium surrounding them, provided, of course, visible chemical action did not take place; and Prof. Exner states that the subsequent experiments of De la Rive, which he thinks proved that chemical action was necessary, negatived those of Pfaff. We have not been able to find any thing about Pfaff in the reference given by Prof. Exner, which is perhaps due to a misprint in the *Annalen*; but Pfaff's letter to Gay-Lussac, which we have come across in the *Annales de Chimie et de Physique*, vol. xli. pp. 236–247, 1829, gives an account of experiments made with as great accuracy as was possible with the instruments in use at the time, and which led to the result that the difference of potential, measured in Volta's way, between zinc and copper metallically connected was not influenced by atmospheres of dry or damp air, oxygen, nitrogen, carbonic-acid gas, hydrogen, and olefiant gas. This is still one of the most valuable papers in the history of contact electricity; and, with all due respect for Prof. Exner's opinion, we are compelled to attach much more faith to the careful work of Pfaff than to experiments of De la Rive, who, we find, actually coated his plates thickly with varnish to prevent oxidation, and, because with such plates he obtained but little difference of potentials, concluded that the well-known Volta effect was produced by oxidation. This experiment, indeed, is only equalled in vagueness by the one described by Prof. Exner at the end of his paper, made with silver in chlorine

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and silver in air, and which, as has been recently pointed out by Prof. Hoorweg, proved nothing but the well-known fact that silver chloride is an electrolyte. For Prof. Exner's arrangement was this:—"A short glass cylinder was closed air-tight at its upper end with a plate of silver, which did not, however, touch the glass, as there was a collar of paraffin-wax. The bottom of the vertical glass tube was closed air-tight with a cork, through which passed two small glass tubes to admit the gas and allow it to escape, and also a platinum wire well insulated by paraffin-wax, and the inner end of which touched the silver plate. This latter was only for making metallic connexion with the condenser.

"A second silver plate, of the same dimensions as the first, could be placed on this condenser in the same way as in the earlier experiments.

"Now, if the condenser-plates were connected, there was naturally not the least charge. But as soon as the interior of the glass tube was filled with dry chlorine, the condenser showed at once a considerable and quite constant electromotive force."

Of course it did, we should reply, seeing that a galvanic cell had been formed of platinum, chloride of silver, and silver. If, however, the platinum wire had been soldered to the silver plate, instead of merely loosely touching it, no such effect would have been observed.

Prof. Exner refers to Mr. Brown's most interesting series of experiments*; but we feel that, although carefully made and faithfully described, they cannot throw much light on the relative values of the so-called contact and chemical theories. For Mr. Brown's zinc and copper plates were coated with zinc sulphide, copper sulphide, or zinc chloride and copper chloride; and every supporter of the contact theory is prepared to admit that there is a difference of potential between a metal and its oxide, chloride, or sulphide, or any other of its salts in a dry or wet state. In fact, in Sir Wm. Thomson's earliest experiments on contact, he refers to the great change produced in the measured difference of potentials of a point in the air close to the zinc, and of a point close to the copper, if the copper be allowed to oxidize†. Had Mr. Brown's apparatus enabled him to make quantitative experiments instead of only qualitative ones, his results would have been of great value in connexion with the electromotive forces of galvanic cells such as we have

* "Theory of Voltaic Action," by J. Brown, Phil. Mag. Aug. 1878, pp. 142–145, and Feb. 1879, pp. 109–111.

† "New Proof of Contact Electricity," by Sir Wm. Thomson, Proc. Lit. and Phil. Soc. of Manchester, Jan. 21, 1862.

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used, in which the ordinary water between the zinc and copper plates is replaced by glass, paraffin-wax, mica, shell-lac, &c.*, or by dry metallic salts, such as have recently been employed by Prof. Hoorweg. Mr. Brown's experiments, however, must not be regarded as confuting Pfaff's work of 1829.

V. We are therefore reluctantly compelled to conclude that Prof. Exner's experiments described in his first paper are inaccurate, and that their striking agreement with his assumption is due to a fortuitous combination of errors. And it may be observed in passing, that the same kind of conclusion has been arrived at by Prof. Young, of Princeton, regarding the result communicated to the Vienna Academy of Sciences, that thermoelectricity is also due to oxidation, since he has recently shown† experimentally that the thermoelectric power of metals is the same in air at a millionth of an atmosphere as at ordinary pressures.

We may mention that, for the purpose of testing whether a gas has any important effect at all in contact action, we have for some time been arranging apparatus to try Volta's original experiments in a Crookes's vacuum.

VI. The succeeding paper of Prof. Exner's was given in the last December number of the Transactions of the Vienna Academy of Sciences, and again in No. 6, B. x. H. 2, pp. 265–284, of Wiedemann's *Annalen*. The ideas contained in it are, we think, based on some misconception of the contact theory of electricity. It has long been known that when the dilute acid of a simple cell is saturated with oxygen there is a primary great electromotive force, which diminishes as the evolved hydrogen begins to collect on the negative plate, not finding any more free oxygen to combine with. It has also been known that, if the metal from either plate is carried over and deposited on the other, the electromotive force of the cell goes down. Prof. Exner, however, sets himself to make careful experiments with a Smee's cell, in which deposition of zinc on the negative plate is rendered impossible by having the plates in separate vessels connected with a glass tube drawn out to a fine point; and he found that the primary electromotive force was 1·15 that of a Daniell's cell, and that it diminished after a current produced by short-circuiting had deposited hydrogen on the platinum plate—also that, whatever was the negative plate, there was the same working electromotive force. "The preceding results," says Prof. Exner, "connect

* "Contact Theory of Voltaic Action," Part II., by W. E. Ayrton and John Perry, Proc. Roy. Soc. No. 106, pp. 26–34, 1878.

† "On the Thermoelectric Power of Iron and Platinum *in vacuo*," by Prof. E. A. Young, Phil. Mag. vol. x. p. 450 (December 1880).

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themselves intimately with the consequences of the chemical theory; they contradict entirely the contact theory." For he reasons:—"The contact theory requires that the electromotive force of a Smee's element should begin with a value 0·73 of that of a Daniell's cell" [this is derived from the heat of combustion of zinc in oxygen, the combination of the oxide with sulphuric acid, minus the energy required for the decomposition of an equivalent quantity of water] "and then sink to a lower value, which depends on the amount of polarization—that is, on the amount of resistance,—and, further, that it depends on the nature of the negative plate. The chemical theory, on the contrary, requires that the Smee's element should first have an electromotive force between 0·732 and 2·15 of that of a Daniell" [2·15 is derived from only considering the heat of combustion of zinc with oxygen and the combination of the oxide with sulphuric acid], "and that this value should fall to 0·732 of a Daniell and then remain perfectly constant, no matter what be the metal forming the negative plate, as long as it does not give rise to chemical changes. Further, the value 0·732 of a Daniell must also depend on the resistance of the element."

Now what is correct in this was all given by Sir Wm. Thomson as far back as 1851; but the greater part is incorrect.

First, it is quite a mistake to say that "the contact theory requires that the electromotive force of a Smee's element should begin with a value 0·73 of that of a Daniell's cell." What the "*summation law*"* of the contact theory says is, that the electromotive force of a complete cell is equal to the algebraical sum of all the differences of potential, each measured separately, at the separate contacts—of metals with any layer of gas on them, gas with liquid, one liquid with another, &c.; and what these latter are in a Smee's water element, for example, can be found from the table at the end of our paper†. For, taking water, we have

Pt	$\mid H_2O$	. .	0·285 to 0·345 volt‡,
H_2O	$\mid$ Zn	. .	−0·156 „ 0·105 „
Zn	$\mid$ Pt	. .	0·981 „ 0·981 „
			1·110 to 1·431 volt,
	Mean	. . .	1·270 volt,

* "Contact Theory of Voltaic Action," Part I., by Professors W. E. Ayrton and John Perry, Proc. Roy. Soc. No. 186, p. 28, 1878.

† "Contact Theory of Voltaic Action," Part III., by Professors W. E. Ayrton and John Perry, Phil. Trans. Roy. Soc. part i. pp. 15–34, 1880.

‡ The variations observed in measuring the electromotive force of contact of a metal and a liquid are due to the effect of gas absorbed, and being

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or about what Exner found it for a dilute sulphuric-acid Smee's cell*.

Secondly, as regards the variations of the electromotive force of a Smee's cell, Sir Wm. Thomson calculated† from Dr. Andrews's experiments that, if we consider the oxidation of the zinc and the combination of this oxide with strong sulphuric acid, minus the energy necessary to decompose the equivalent quantity of water, we obtain as the electromotive force of a Smee's cell 2,056,200 British absolute units, which corresponds with 0·82 Daniell [with more modern data Exner gives 0·73], since the electromotive force of a Daniell's cell as determined experimentally by Joule is 2,507,100 British absolute units. Sir William adds:—"It is to be remarked that the external electromotive force determined for a single cell of Smee according to the preceding principles, by subtracting the chemical resistance" [this latter being due to the evolution of hydrogen upon the platinized silver] "from the value $J\theta e$, is the *permanent working* external electromotive force. The electrostatical tension which will determine the initial working external electromotive force depends on the primitive state of the platinized silver plate. It could never be greater than to make the initial working force $J \times 1670 \times e$, or 5,444,500" [2·17 Daniells according to the above reduction], "corresponding to the combination of zinc with gaseous oxygen, and of the oxide with sulphuric acid. It might possibly reach this limit if the platinized surface had been carefully cleaned and kept in oxygen gas until the moment of immersion, or if it had been used at the positive electrode of an apparatus for decomposing

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absorbed, by the liquid &c., and will always be noticed if the measuring-apparatus be only sufficiently delicate. Such differences have nothing in common with the great discrepancies referred to at the commencement of this note, on the results obtained by Prof. Exner and by other experimenters for the electromotive force at the contact of metals, since the latter is fairly constant at a constant temperature, so that any great discrepancies in such measurements can only arise from errors in experimenting.

* We are unable, from our results at present published, to calculate exactly the electromotive force of Smee's dilute-acid cell, since, as regards liquids, it was only with distilled water and with strong acids that the electromotive force of contact of platinum was measured in our experiments. We can, however, from these latter approximate to limiting values of the electromotive force of a Smee's dilute-acid cell; that is, using the values of the contact electromotive forces of platinum with water and with strong sulphuric acid, and assuming that the contact of platinum with dilute sulphuric acid is somewhere between these two, it follows that the electromotive force of a Smee's dilute-acid cell, as determined from the sum of the separate contact differences of potential, must at first be greater than 1·567 volt.

† "On the Application of Mechanical Effect to the Measurement of Electromotive Forces," by Prof. Thomson, Phil. Mag. Dec. 1851.

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water immediately before being connected with the zinc plate; and then it could only reach it if the whole chemical action were electrically efficient, and if there were no chemical resistance due to the affinity of the platinized surface for oxygen.

"It is also to be remarked, that the permanent working electromotive force of a galvanic element consisting of zinc and a less oxidizable metal immersed in sulphuric acid can never exceed the number 2,056,200, derived from the *full* thermal equivalent for a single cell of Smee's, since the chemical action is identical in all such cases, and the mechanical value of the external effects can never exceed that of the chemical action. In a pair consisting of zinc and tin, the electromotive force has been found by Poggendorff to be only about half that of a pair consisting of zinc and copper, and consequently less than half that of a single cell of Smee's."

The alteration in the electromotive force of galvanic cells produced by gas dissolved in the liquid has been known for a long time, indeed before the experiments of De Fonville, De Lerain, Marié-Davy, referred to by Prof. Exner; and we may draw attention to some experiments of our own, which show that the polarization phenomena in voltameters are wonderfully influenced by the state of the platinum plates and the liquid as regards absorbed gas*.

Thirdly, Prof. Exner states that the contact theory requires that the permanent electromotive force of a Smee's should depend on the nature of the negative plate, while the chemical theory requires that it should be independent of the negative metal; further, that his experiments agree with the latter conclusion and disagree with the former. Now the first part of this statement is wrong; for the contact theory does not require *per se* that the electromotive force should, or should not, depend on the negative metal. What the contact theory tells us is that the electromotive force of the cell will depend on the electromotive force of contact of every pair of dissimilar substances in the circuit, and therefore will depend, among other things, on the contact difference of potentials of the negative metal with possibly a gas, the gas with the liquid, the liquid possibly with another gas, the gas with the positive metal, and the positive metal with the negative. Further, the conclusion itself to which Prof. Exner has arrived, viz. that the working electromotive force of a Smee's cell does not depend on the negative plate, has, like his other conclusions, been disputed. For Beetz† has recently published the results of

* "A Preliminary Account of Reduction of Observations on Strained Material," &c., by John Perry and W. E. Ayrton, Proc. Roy. Soc. No. 30, pp. 411–435 (1880).

† "On the Nature of Galvanic Polarization," by W. Beetz, *Annalen der Physik und Chemie*, B. x. H. 3, pp. 348–371 (1880).

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experiments with simple cells; and his electromotive forces are:—

Zinc and platinum.		Zinc and copper.		Zinc and silver.	
Open.	Closed.	Open.	Closed.	Open.	Closed.
1·52	0·72	0·98	0·46	1·23	0·57

And when he used sodium amalgam the difference of the electromotive forces of the closed cells was equally striking :—

Sodium amalgam and platinum.		Sodium amalgam and silver.		Sodium amalgam and copper.		Sodium amalgam and zinc.	
Open.	Closed.	Open.	Closed.	Open.	Closed.	Open.	Closed.
2·31	1·33	2·05	1·22	1·79	1·14	0·78	0·68

The nature of the negative metal must always have a great effect on the initial electromotive force; we are, however, astonished that Prof. Beetz should find such great differences in the columns headed "closed." Our impression has always been that when the negative metal becomes sufficiently coated with hydrogen, the contact between the thin conducting-layer of gas and the metal was almost like that of a pair of metals; and if this is the case, then

$$H \mid Pt + Pt \mid Zn$$

is the same as

$$H \mid Cu + Cu \mid Zn,$$

and almost any negative metal would eventually act like a conducting hydrogen plate.

Prof. Exner's second paper, like his first, we are compelled to regard as wrong theoretically, and describing experiments which do not lead to the conclusions he has deduced from them.

It is not necessary for us to follow Beetz into his disproof of Prof. Exner's statements regarding the use of the word polarization, which is employed in England in the vaguest way to indicate a change of current whether from alteration in the electromotive force or in the resistance, but to which in Germany various philosophers attach distinct meanings. Between the contact theory as properly understood and the chemical theory there is really nothing antagonistic. The "summation law" deduced from various experiments for metals and one liquid, and extended by our own experiments

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to two or more liquids, tells us that the electromotive force of any arrangement is equal to the algebraical sum of all the separate differences of potential at the various surfaces of contact of dissimilar substances, each difference of potential being measured by an induction method such as we have described and used. The chemical theory tells us that the energy given out in any electric circuit must be equal to the consumption of chemical energy in the circuit. The electromotive force, then, in any circuit can either be calculated on the contact theory, if we know *each of all* the differences of potential at the various surfaces of separation of dissimilar substances (solid, liquid, or gaseous) in the circuit, or it may be calculated on the chemical theory, if we know exactly what are *all* the physical and chemical changes taking place and the heat-equivalent of every one of them. And the amount of the electromotive force determined in either of these two ways must be identical.

The question of the relative delicacy of the two methods is a totally different question. Any balance accurately made will weigh accurately; but one form of chemical balance is more delicate than another: so, in the same way, there is every reason for believing that an electric test is a far more delicate test of chemical action than the analytical methods employed by the chemist. For example, the action of even small quantities of paraffin-wax on metal, which would quite escape the test of a chemical analysis, we have not only detected, but even measured, with an electrometer*.

VII. *Specific Refraction and Dispersion of Isomeric Bodies.* By J. H. Gladstone, *Ph.D., F.R.S.*†

AMONG the properties of a body which are least liable to change, and which are the most capable of throwing light on its molecular constitution, is its specific refraction. This is the refractive index minus unity divided by the density, or $\frac{\mu-1}{d}$.

In early papers on the subject‡, this specific refraction (or "specific refractive energy," as it was then called) was shown to be a constant, unaffected, or nearly so, by change

* "Contact Theory of Voltaic Action," Part II., by Profs. W. E. Ayrton and John Perry, Proc. Roy. Soc. No. 186, pp. 26–34, 1878.

† Communicated by the Physical Society, having been read at the Meeting on November 27, 1880.

‡ Phil. Trans. 1863, p. 323, and 1869, p. 9; Journ. Chem. Soc. 1865, p. 108; Landolt, Pogg. *Ann.* cxxii. p. 545; Wüllner, *ibid.* cxxxiii. p. 1.

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of temperature, by the passage from the liquid to the solid or even gaseous condition, by mixture with other liquids, or by solution, or even (within certain limits) by changes of chemical combination.

In regard to changes of temperature, however, it was observed at the very commencement that there was "some influence, arising wholly or partially from dispersion,"* which interferes with the exactitude of the law. In order to obviate this, if possible, the calculations were made for the least-refrangible limit of the spectrum, according to the formula of Cauchy. But it was found that these gave results little, if at all, more exact than those for the line A of the solar spectrum. In our subsequent work, Mr. Dale and I did not consider it worth while to go through the labour of this calculation; and my observations are always reckoned, if possible, for the line A, whilst Landolt has preferred the α of hydrogen gas, which is identical with the solar line C.

In regard to the passage from one state of aggregation to another, the few cases that have been directly observed are fairly in accordance with the law†.

In the case of mixtures of liquids‡, the conclusion that the specific refraction of a mixture of liquids is the mean of the specific refractions of its constituents is a near approximation, if not an absolute truth.

With regard to the influence of solution, some doubt has recently been thrown upon the deductions drawn from dissolved salts or other chemical compounds; but the general, if not absolute, correctness of the method is supported by a very large amount both of direct proof and collateral evidence §. As an instance of the latter, I may quote the last observations made in my laboratory. The specific refraction of pyrene, $C_{16}H_{10}$, as determined from its solution in benzol, in chloroform, and in carbon disulphide, is respectively

·6235, ·6252, ·6240,

a practical agreement which could scarcely happen if the method were erroneous. Taking the mean of these numbers, and multiplying it by the atomic weight, we obtain, as the refraction-equivalent of pyrene, the very high figure of 126·1.

In regard to chemical combination, it is now well understood that an elementary substance such as carbon will exert the same retarding influence upon the rays of light throughout

* Phil. Trans. 1863, p. 323. † Phil. Trans. 1869, pp. 10, 11.

‡ Phil. Trans. 1863, p. 332; Landolt, *Ann. Chem. Pharm.* iv. (Sup. Bd.) 1865, p. 1.

§ Phil. Trans. 1869, pp. 14, 15.

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a very large number of its compounds, whilst in others it may exert a different influence. This has naturally attracted the attention of organic chemists to the subject, and promises to be a very fruitful field of investigation.

In the present communication I intend to confine my remarks to those groups of bodies which exhibit isomerism. It is well known that two or more compound bodies, differing from one another in physical or chemical properties, may be composed of the same elements in precisely the same proportions. In such a case the specific refraction may be the same, or it may be different.

It is found to be the same notwithstanding differences of other optical properties, differences of molecular weight, and differences of chemical properties.

1st. *Differences of other optical properties.*—It so happens that among the bodies which display the phenomena of circular polarization, there are several isomeric groups, and that different members of these groups differ from one another in their effect on the polarized ray. Thus with respect to the different terpenes, $C_{10}H_{16}$, some of them rotate the plane of polarization to the right and others to the left, and that to different degrees; yet they have all practically the same specific refraction, 0·537*.

It was also an early observation that solutions of cane-, grape-, and honey-sugar, and gum, of the same strength, though they differ largely in their effect on polarized light, give the same, or very nearly the same, refraction and dispersion†.

Again, it is well known that tartaric acid is an optically active substance, and racemic acid optically inactive, while their chemical composition, $C_4H_6O_6$, is identical. The following table gives the specific refraction and dispersion of these two acids in solution as deduced from data previously published‡. The specific dispersion is the difference between the specific refraction for the line A and that for the line H, or, which is the same thing, $\frac{\mu_H - \mu_A}{d}$.

	Specific refraction.	Specific dispersion.
Tartaric acid.....	·3020	·0131
Racemic acid.....	·3036	·0143

Carvol and menthol, the odorous principles of caraway and

* Chem. Soc. Journ. 1864, p. 18. † Phil. Trans. 1863, p. 332.
‡ Ibid. 1869, p. 30.

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mint respectively, are very definite substances, of the composition $C_{10}H_{14}O$; but the first rotates the plane of polarization very powerfully to the right, and menthol very powerfully to the left; yet they have practically the same specific refraction *.

	Specific gravity.	Specific refraction.	Specific dispersion.
Carvol from caraway	0·9530	·5126	·0362
„ from dill	0·9562	·5115	·0348
Menthol	0·9394	·5100	·0331

2nd. *Differences of molecular weight.*—Interesting cases of this are to be found among the essential oils, as shown in the subjoined table, in which the numbers given for the *terpenes* are the mean of ten different specimens, for the *citrenes* of twelve specimens, and for the *cedrenes* of eight specimens, each group probably consisting of several isomerides †.

	Specific gravity.	Specific refraction.	Specific dispersion.	Refraction-equivalent.
Terpenes, $C_{10}H_{16}$	0·8600	·5370	·0302	73·03
Citrenes, $C_{10}H_{16}$	0·8466	·5475	·0336	74·46
Cedrenes, $C_{15}H_{24}$	0·9166	·5392	·0307	110·00
Colophene, $C_{20}H_{32}$	0·9391	·5413	·0329	147·23

The terpenes and citrenes show a constant though not a large difference in specific refraction; but the polymeric bodies of the same series seem to have so nearly the same specific refraction as the terpenes that the differences may be attributed to impurities or experimental error. The refraction-equivalents of the three groups are of course very nearly in the proportions of 2, 3, and 4.

3rd. *Differences of chemical properties.*—In the cases already mentioned it is possible that the differences of physical properties depend upon some difference in the arrangement of the ultimate atoms; but there are other cases in which this is perfectly well known to be the case. Isomeric bodies of this nature are more strictly termed metameric. Delffs showed, as far back as 1850‡, that such pairs as formic ether and acetate of methyl, both of the ultimate composition $C_3H_6O_2$, had the same refraction; whilst the papers of Landolt, and the more recent ones of Brühl §, abound in instances. Among the observations in my notebook, hitherto unpublished, occur the following:—

* Chem. Soc. Journ. 1872, p. 9.
† Chem. Soc. Journ. 1864, p. 18.
‡ Pogg. *Ann.* lxxxi. p. 470.
§ Liebig's *Ann.* cciii. pp. 1, 255.

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	Specific refraction.	Specific dispersion.
Propyl iodide, C_3H_7I	·2844*	·0216
Isopropyl iodide, „	·2883*	
Cresol from thymol, C_7H_8O	·5116	·0454
Metacresol, „	·5091	·0452
Benzylic alcohol, „	·5069	·0415
Nitrobenzoic acid (α), $C_7H_5(NO_2)O_2$	·3994	
„ „ (β), „	·4004	
Benzylic butyrate, $C_{11}H_{14}O_{12}$	·4777	·0332
„ isobutyrate, „	·4805	·0331
Monochloro-toluol, C_7H_7Cl	·4807	·0409
Benzyl chloride, „	·4836	·0409

Though identity of ultimate composition will generally produce identity of specific refraction, there are certain cases in which isomeric (or, rather, metameric) bodies differ widely in their power of retarding the transmitted ray. The earliest of those observed was the case of aniline and picoline, C_6H_7N; and as Dr. Thorpe has kindly lent me his specimen of picoline, I am able to confirm the previous determinations.

	Specific refraction.	Specific dispersion.
Aniline	·550	·0635
Picoline (*C. G. Williams*) ...	·513	·0448
„ (*Thorpe*)	·522	·0431

The difference in this pair was originally attributed to the fact that the two bodies "are constructed very differently;" and in subsequent papers it was pointed out that, when the atoms of carbon were not saturated in the usual way, there is an augmentation in their refractive power, analogous to what happens when an element such as iron changes its valency †. Brühl has expressed this more definitely by putting forward the theory that, whenever two carbon atoms are doubly linked, there is an increase of the refraction-equivalent amounting to 2·0 for the limit of the spectrum, as reckoned by Cauchy's formula, or about 2·2 for the hydrogen-line α, and of course a little less than 2·2 for the line A. I have no doubt that both Brühl himself and other chemists will have much to say in the future as to the extent to which this theory may be in accordance with present views of the constitution of the various organic compounds.

* Brühl gives the values 0·2873 and 0·2907 respectively for the line C.

† Phil. Trans. 1863, p. 333; Chem. Soc. Journ. 1870, p. 150; Proc. Royal Instit. March 1877.

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In my notebook I find the following instances of isomeric bodies which differ in specific refraction :—

		Specific refraction.	Specific dispersion.	Refraction-equivalents.
Acetone,	C_3H_6O.....	·4420	·0207	25·64
Butyric ether,	$C_6H_{12}O_2$	·4402	·0191	51·06
Allyl alcohol,	C_3H_6O.....	·4734	·0275	27·45
Carvol,	$C_{10}H_{14}O$	·5122	·0355	76·83
Thymol,	„	·5206	.....	78·09
Nitraniline (α),	$C_6H_6(NO_2)N$	·469	.....	64·72
„ (β),	„	·546	.....	75·34
Cresylic acetate,	$C_9H_{10}O_2$	·4677	·0348	70·15
Benzylic acetate,	„	·4945	·0394	74·17
Phenyl-ethyl acetate,	$C_{10}H_{12}O_2$.....	·4776	.....	78·32
Hydrocinnamene acetate,	„	·5183	·0382	85·00

Acetone and allylic alcohol have been compared already by Brühl, with similar results, and form one of the striking cases on which his theory depends. The refraction-equivalent of thymol being 6·8 above the normal, is confirmatory of the position which is now given to it in the great aromatic group—a position which I was disposed to assign to it ten years ago, on the evidence of its optical properties.

The two nitranilines were lent to me by Dr. Mills. Unfortunately they are very sparingly soluble bodies; and the above figures, though each of them the mean of three observations, are only given as approximately correct. But that they are really different there can be no doubt.

The two pairs of acetates were lent by Dr. Hodgkinson; and the last one is of particular interest, on account of the cinnamene compound having a higher refraction-equivalent than can be accounted for by any existing theory.

Specific Dispersion.

Though there are several sources of error in determining indices of refraction for the line A or for the line H, the actual distance between these two may be measured with great exactness, and the experimental error rarely, I believe, exceeds 0·0002. The possible error in the determination of the density at the same temperature has also to be considered with regard to specific dispersion; but any inaccuracy in the figures above given will certainly be confined to the fourth place of decimals.

Among the conclusions that may be drawn in regard to dispersion are the two following :—

1st. Where the carbon atoms are exerting a more than normal influence on the refraction of the rays of light, there is

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a great increase of dispersion. It was pointed out some years ago that the aromatic bodies have a great dispersive power*, and that "dispersion, as well as refraction, increases very rapidly with the number of atoms of carbon that are not combined with at least two of hydrogen or their equivalent."† Evidence of this is to be found both in the older and the more recent researches, whether in this country or on the Continent, and is always accumulating. Confining our attention to the figures given in this paper, it will be observed that in cases where the carbon is normal the specific dispersion is expressed by low figures (the highest of which is, in fact, acetone, ·0207); while in the isomeric allylic alcohol, which has a higher refraction, it rises at once to ·0275. The essential oils and their congeners, and such bodies as cresylic acetate, are above ·0300; while the purely aromatic compounds are all above ·0400.

2nd. Where isomeric bodies have the same or nearly the same specific refraction for the line A, they have the same also for the line H. The apparent deviations from this rule I am disposed to attribute to experimental error, and still more to impurity of substance. It is clear that in many cases of carbon compounds the presence of a differently constituted body would reveal itself by its influence upon dispersion more than upon refraction.

VIII. *On the Specific Volumes of Oxides.* *By* Bohuslav Brauner, *Ph.D.*, *and* John I. Watts, *Owens College*‡.

THE researches of Persoz, Karsten, Filhol, Kopp, Schröder, Löwig, Schafarik, Playfair and Joule, Baudrimont, Heimroth, and others have yielded a considerable supply of material relating to the specific volumes of many bodies, especially to those of the oxygen compounds; and by aid of these results many interesting theories can be conceived. It was, however, Kremer§ who first pointed out the regularities which the volumes of the oxides of the natural groups of elements exhibit. But after the demonstration of the Periodic Law by Mendelejeff this question considerably developed in importance, because the relations of the various members of the natural groups to one another were made more strikingly apparent. Mendelejeff himself‖ points out the regularities which the specific volumes of the oxides exhibit in the different groups of the system; but he only followed this out in one

* Journ. Chem. Soc. 1870. † Proc. Roy. Inst. March 1877.
‡ Communicated by the Authors. § Pogg. *Ann.* cxxx. p. 77.
‖ *Ann. Chem. Pharm.* Suppl. viii. p. 143; Chem. News, xl. p. 255, xli. p. 49.

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single series. He touches upon it, however, shortly, in his Russian 'Principles of Chemistry' (vol. ii. p. 857). We have not yet been able to consult his papers on this subject, published from 1858 to 1865 in the Russian 'Mining Journal.'

In Table I. we give an arrangement of all the oxides which are at present known, in which the number of oxygen atoms correspond to the numbers of Mendelejeff's groups. Some of these numbers have been obtained from estimations made by us by means of a very accurate method, which has been already described and used by one of us *.

TABLE I.—Specific Volumes of the Oxides.

Groups	I.	II.	III.	IV.	V.	VI.	VI.
Series.	$\frac{1}{2}R_2O$.	$\frac{1}{2}R_2O_2$.	$\frac{1}{2}R_2O_3$.	$\frac{1}{2}R_2O_4$.	$\frac{1}{2}R_2O_5$.	$\frac{1}{2}R_2O_6$.	$\frac{1}{2}R_2O_7$.
1.	H 10						
2.	7 *Li*	8 *Be* 8	19 *B*	40? *C*	*N*	*O*	*F*
3.	11 *Na* 11	12 *Mg* 12	Al 13	Si 23	P 30	S 41	Cl
4.	17 K	18 Ca	18 Sc	20 Ti	26 V	37 Cr	Mn
5.	Cu 12	Zn 14	Ga (17)	— (23)	As 31	Se —	Br
6.	(21) Rb	22 Sr	23 Y	23 Zr	30 Nb	33 Mo	—
7.	Ag 14	Cd 16	In 19	Sn 22	Sb 42	Te —	I
8.	(25) Cs	28 Ba	25 La	26 Ce			
9.							
10.	.....	.....	.....	.....	31 Ta	32 W	
11.	Au (18)	Hg 19	Tl (23)	Pb 27	Bi 42	Ng? †	
12.	.....	.....	.....	21 Th	.....	56 U	

Preparation and Specific Gravity of Lithium Oxide.—The lithium oxide used for the sp. gr. determinations was prepared by strongly heating pure lithium carbonate with a slight excess of charcoal. Lithium oxide is formed according to the equation

$$Li_2CO_3 + C = Li_2O + 2CO.$$

This mode of preparation, however, is not to be recommended, because the Li_2O dissolves platinum. In our observations we estimated the sp. gr. of the insoluble residue, and made a correction for it.

Mean sp. gr. of Li_2O at 15° C. (corrected for insoluble residue) = 2·102; sp. vol. = 7·12.

We attempted to prepare Li_2O in a pure state by heating the nitrate in a silver basin; but we obtained a very impure product containing Li_2CO_3 and silver.

Specific Gravity of Bismuth Pentoxide.—A sample of hydrated Bi_2O_5 was obtained from Mr. M. M. P. Muir, and heated to 120° C. until it ceased to lose weight.

* Thorpe and Watts, Journ. Chem. Soc. Feb. 1880, p. 102.

† This place will be probably occupied by the metal Norvegium, Ng = 214, if it forms, besides the oxide Ng_2O_3, a peroxide of the formula NgO_2.

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I. Sp. gr. of Bi_2O_5 = 5·917.
II. „ „ „ = 5·919.

Specific Gravity of Uranic Oxide.—One sample of UO_3 was prepared by heating the nitrate to 280°. A second one was prepared by heating the hydrated oxide to 300°.

Sp. gr. of sample No. 1 = 5·26, 2 = 5·02 } Mean = 5·14 ; sp. vol. 56·03.

In explanation of Table I. it is to be borne in mind that the numbers refer to one atom of the metal in the form of oxide, in order to obtain comparable numbers. Many of the numbers are taken from the latest papers of Nilson and Petterson *, and are found to give values for specific volumes somewhat smaller than those derived from older observations.

We only give the above values as approximate ones, because the material to work upon does not allow of great accuracy; and we only take the first seven groups into consideration, because the specific gravities of oxides of the eighth, with the formula RO_4, are not known.

A glance at the table shows us that the specific volume increases steadily along the horizontal as well as down the vertical lines. The difference between the even and the odd series is particularly well defined. Some of the "typical elements," especially Li, Be, Na, and Mg, can be used as representatives of both series. The specific volume of an oxide, where it is at present unknown, can be calculated from data obtained from the values surrounding it.

Of all the oxides which are at present known, that of lithium possesses the smallest specific volume, the metal having the least atomic weight. On the other hand, uranic oxide, UO_3, has the largest atomic volume, uranium possessing the greatest atomic weight.

A remarkable increase of volume is exhibited by antimony and bismuth pentoxides. It should nevertheless be mentioned that Sb_2O_5 seems to exist in two allotropic forms, which possess quite different specific gravities. One has the sp. gr. 6·525 (Boullay), and the other 3·779 (Playfair and Joule)†. Accordingly the specific volume is either 25 or 42.

It appears likely that two similar modifications of other oxides exist—of bismuth, for example, and of uranium oxide, the second one possessing the specific volume about 30. Others of the numbers given are still uncertain, that for BaO for example. The values at present given for its sp. gr.

* *Ber. deut. chem. Gesell.* xiii. p. 1459.

† Clarke, 'Constants of Nature,' No. I. Our other values are also mainly derived from this source.

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extend from 4·0 to 5·456; and accordingly the atomic volume varies from 28·1 to 38·3. An oxide of barium of sp. gr. 6·4, giving sp. vol. 24, might be expected to exist.

Some interesting numbers are obtained if we calculate the volume which one atom of oxygen possesses in the various oxides. To commence with, we suppose that the metal does not alter in volume when it combines with oxygen, we subtract the specific volume of the metal from that of the oxide. We quote the values from Lothar Meyer's 'Modern Theory of Chemistry,' 4th edit. 1880, p. 141. To take an example,

$\frac{1}{2}Na_2O = 11{\cdot}1$, $Na = 23{\cdot}7$, $11{\cdot}1 - 23{\cdot}7 = -12{\cdot}6$ for $\frac{1}{2}O$,

or $-25{\cdot}2$ for O, and so on.

The following Table contains the corresponding numbers when the elements are arranged according to the periodic law.

TABLE II.

Specific Volume of one Atom of Oxygen in the Oxides.

Groups	I.	II.	III.	IV.
Series.	R_2O.	R_2O_2.	R_2O_3.	R_2O_4.
1.	*H*			
2.	− 9·6 *Li*	+ 2·7 *Be*	+8 *B*	+21·2 *C*
3.	−25·2 *Na* −25·2	− 1·8 *Mg* −1·8	Al +1·4	Si + 5·7
4.	−55·4 K	− 7·2 Ca	(−0·7) Sc	(+3) Ti
5.	Cu+10·4	Zn +5·1	Ga (+2·9)	— (+5)
6.	(−70) Rb	−12·9 Sr	(−0·7) Y	+0·8 Zr
7.	Ag+7·6	Cd +5·5	In +2·5	Sn +3·2
8.	(−84) Cs	− 8·5 Ba	+1·7 La	+2·2 Ce
9.				
10.	.....	.....	.....	.....
11.	Au (+5)	Hg +4·7	Tl (+4)	Pb +3·7
12.	.....	.....	.....	−1·8 Th

Groups	V.	VI.	VII.
Series.	R_2O_5.	R_2O_6.	R_2O_7.
1.			
2.	*N*	*O*	*F*
3.	P +6·6	S +8·3	Cl
4.	+6·6 V	+ 9·3 Cr	Mn
5.	As +7·0	Se	Br
6.	+6·6 Nb	+ 7·2 Mo	—
7.	Sb +9·5	Te	I
8.			
9.			
10.	+4·8 Ta	+ 9·0 W	
11.	Bi +8·4	Ng ?	
12.	.....	+14·3 U	

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We deduce the following :—

1. In strong bases the oxygen possesses a negative volume.

2. In the oxides of the heavy metals and metalloids the volume of the oxygen is positive.

3. The earth-metals unite with oxygen without any appreciable change of volume; and on this account they form a connecting link between acids and bases.

4. The higher the specific volume of the metal in the oxide, the more negative is the specific volume of the oxygen combined with it; for instance

Sp. vol. K = 45·4,
Sp. vol. O = 55·4.

The lower the specific volume of the element, the more positive is the specific volume of the oxygen in the oxide ; for example

Sp. vol. C = +3·6,
Sp. vol. O = +21·2.

Mendelejeff has published some very interesting observations of this kind in his Russian 'Principles of Chemistry,' pp. 856–859. He gives a very beautiful hypothesis of the mechanical theory of the act of combination. It is not, however, suitable to enter upon these discussions here.

When we observe the above arrangement in Table II, we notice the following :—The more strongly electro-positive the base which an element forms with oxygen, the greater is the negative value for the volume of the oxygen. It appears as if the negative maximum were reached with cæsium. On the contrary, the maximum of positive values is probably attained in the vicinity of fluorine. From this one perceives that the value representing the atomic volume of the oxygen in the oxides can, to a certain extent, be looked upon as the amount of affinity of the metal for the oxygen.

In the same manner that, in the horizontal series of the periodic system, the electro-negative character increases with the atomic weight, the volume of the oxygen changes from negative to positive. The opposite appears to be the case in the vertical groups—at all events, as regards the even elements. In this matter new researches upon the metals and their oxides would be of considerable interest.

We cannot conclude without thanking Mr. M. M. Pattison Muir, of Caius College, Cambridge, for his great kindness in supplying us with the material to make our determinations of the specific gravity of bismuth pentoxide.

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IX. *On the Atomic Weight of Beryllium.*

By BOHUSLAV BRAUNER, *Ph.D., of Prague**.

THE question whether beryllium is divalent ($Be'' = 9{\cdot}1$) or trivalent ($Be''' = 13{\cdot}65$) is still a matter of interest for chemists, although Nilson† states that this question may be regarded as completely settled in favour of *trivalent* beryllium.

As this point is a vital one as regards the truth of the periodic law, I venture to ask attention to the following remarks.

The history of this question will be found in the papers of Nilson and Pettersson‡, as well as in that of Humpsidge§; and here I will mention only the most important points.

In the year 1878 Messrs. Nilson and Pettersson found the specific heat of beryllium to be 0·4083 between 0° and 100°; and, in accordance with the law of Dulong and Petit, they concluded that the atomic weight of beryllium is $=13{\cdot}8$, and its oxide represented by the formula Be_2O_3.

Soon afterwards the author of the present paper‖ published a note, in which he pointed out that the following elements, possessing small atomic weights, form an exception at ordinary temperature to the law of Dulong and Petit:—boron, carbon, magnesium, aluminium, silicon, phosphorus, sulphur, and (in compounds in the solid state) hydrogen, nitrogen, oxygen, fluorine. The writer concluded that in all probability beryllium (9·2), which stands close to boron (11), possesses an atomic heat of 3·8 instead of 6·4, and that it must be placed among the eleven exceptions already mentioned. At the same time it was remarked that the normal value of the atomic heat may probably be reached at a somewhat higher temperature. If, on the other hand, beryllium is considered to be $B''' = 13{\cdot}8$, this metal cannot be placed in Mendelejeff's series.

Nilson and Pettersson conclude, from their recent experiments, that beryllium does not show any extraordinary rise of atomic heat at a high temperature, and that it therefore does not form an exception to the law of Dulong and Petit. The same authors have also lately published a valuable research on the molecular volumes and the molecular heats of the rare

* Communicated by the Author.

† *Berliner Berichte*, xiii. p. 2035.

‡ Wiedemann's *Annalen*, iv. p. 554; *Berl. Berichte*, xi. pp. 381 & 906, xiii. p. 1451.

§ Chemical News, xlii. p. 261.

‖ *Berliner Berichte*, xi. p. 872.

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earths and their sulphates; and they find therein a further confirmation of their views respecting beryllia.

The first part of the paper of Messrs. Nilson and Pettersson has been thoroughly discussed by Professor Lothar Meyer*, who showed that from their own experiments a conclusion opposite to that of the above-named authors may be drawn, viz. that beryllium is a dyad, inasmuch as it belongs to a class of elements in which the specific heat increases with the temperature, but in which the increment for 1° C. regularly diminishes, the opposite of this being the case with metals obeying the law of Dulong and Petit. Lothar Meyer concludes therefrom that beryllium is a dyad, $Be'' = 9{\cdot}1$.

The aim of the present communication is to discuss the second part of the researches of Messrs. Nilson and Pettersson. These experimenters find that beryllia possesses a molecular heat and molecular volume nearly equal to those of the other rare earths; and they go on to state that, if the formula BeO for beryllia be accepted, values for the above relations are obtained which are without analogy in the whole range of the science.

On this point I would beg to remark:—

(1) If we consider beryllia as BeO, its *molecular volume* is 8·3. And if the oxides be arranged according to the periodic law (see the preceding paper), this number exactly corresponds to the position of a dyad beryllium. For beryllium thus stands between lithium and boron. The same is the case if we consider the vertical groups

I.	II.	III.
Li 7	Be 8	B 19
Na 11	Mg 12	Al 13
K 17	Ca 18	Sc 18

If we consider the specific volume of beryllia, compared with the same quantity of oxygen as that contained in alumina, we get the same number as would be given by the supposed oxide of trivalent beryllium, $\frac{1}{2}Be_2O_3$. We get the number 12·5, which is nearly equal to that of $\frac{1}{2}Al_2O_3 = 13$. But in the same way the double volume of lithia, $2 \times 7 = 14$, is nearly equal to that of magnesia $= 12$. The same relations are shown by the oxides of boron and silicon; for $\frac{2}{3}$ vol. B_2O_3 (25·3) = 1 vol. SiO_2 (23). But the explanation of this relation is given by the equation

$$Li : Mg = Be : Al = B : Si,$$

following from the periodic law.

* *Berliner Berichte*, xiii. p. 1780.

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(2) Let us consider the numbers which express the volume of oxygen in the oxides (see Table II. of the preceding paper). Beryllium, with the number +2·7, again finds its right place between lithium (−11) and boron (+8). Further, we see that beryllia is the most feeble base in the group of the alkaline earths :—

BeO	MgO	CaO	SrO	BaO
+2·7	−1·8	−7·2	−12·9	−8·5

(3) Let us next examine the specific volumes of the sulphates of the first three groups. We find numbers in this way corresponding closely with the position of beryllium II. −2 (Mendelejeff). The specific volumes are referred to one atom of sulphur in the molecule, in order to get comparable numbers:—

I.	II.	III.	
Li	Be	B	
49·8*	43·0	—	
Na	Mg	Al	
54·6	44·3	42·2	
K	Ca	Sc	
67·0	46·6	48·6	
	Zn 45·6		
	Sr	Y	
	47·1	59·6	
Ag 76·3	—		In 49·9
	Ba	La	
	51·8	52·4	

If beryllium were placed in the third group as a triad before aluminium, the volume of its sulphate ought to be smaller by some units than 43, *i. e.* about 40. But meanwhile, from the equation

$$\mathrm{Li} : \mathrm{Mg} = \mathrm{Be} : \mathrm{Al}$$

it follows that the volumes of the sulphates of every couple of those metals will not have very different values, as indeed we have seen in the case of the oxides.

(4) Let us next take the *molecular heats* of the oxides which have the number of oxygen atoms corresponding to the numbers of Mendelejeff's group. Expressing the molecular heats with respect to one atom of metal in the oxide, we get the following numbers:—

* Deduced from the specific gravity, which I found to be 2·21 at 15° C.

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	I.	II.	III.	IV.	V.	VI.
2.	Li (5)	Be 6·2	B 8·3			
3.	...	9·7 Mg	9·4 Al	11·5 Si		
4.	...	Ca 10·0	Sc 10·4	Ti 14·0		
5.	...	10·1 Zn	9·8 Ga			
6.	...	.....	Y 11·6	Zr 13·3	.....	Mo 19·1
7.	...	.....	11·1 Zn	14·0 Sn		
8.	...	.....	La 12·2	Ce 15·0		
9.						
10.	...	.....	.....	.....	.....	W 18·5
11.	...	11·2 Hg				
12.	...	.....	.....	Th 14·5		

These show that the so-called Neumann's law forms but an approximate expression for the relations of specific heat to composition, and is therefore not of such cardinal importance for the solution of the present question as Messrs. Nilson and Pettersson assume. On the contrary, those gentlemen have themselves furnished us with the experimental materials for establishing the following proposition:—"The oxides of *one natural group* have nearly the same molecular heats; but these *rise* with increasing atomic weight." Further investigations must show if this is also the case in the horizontal series.

The molecular heat of the supposed trivalent beryllium is here seen to approach nearly to that of alumina; for

$$\tfrac{2}{3}BeO = \tfrac{1}{2}Be_2O_3 = 9{\cdot}30, \quad \tfrac{1}{2}Al_2O_3 = 9{\cdot}39;$$

but also

$$\tfrac{2}{3}B_2O_3 = 11{\cdot}1, \quad SiO_2 = 11{\cdot}5.$$

In the same way the molecular heat of lithia, $\frac{1}{2}Li_2O$, may be calculated to be about 5.

The smallest molecular heat will correspond to the smallest volume of Li_2O, and the next larger values will be shown by beryllia. The three above-mentioned relations again find their expression in the equation

$$Li : Mg = Be : Al = B : Si.$$

In the Russian edition of his 'Principles of Chemistry' (vol. ii. p. 710 *et seq.*) Mendelejeff* has speculated on the possible reason of the fact that several compounds possess a smaller molecular heat than others. He arrives at the conclusion that those solid bodies whose molecule may be supposed to be a more complicated one, have a smaller specific heat than those which possess a less complicated structure. On this the following remarks may be made.

* See also Journal of the Russian Chemical Society, 1873.

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Let us consider the molecular volumes, *i. e.* the relative volumes of the single chemical molecules and their relations to the unit volume, *i. e.* the inverse values of molecular volumes if we assume the highest volume as a unit. If we compare these values with the inverse values of the molecular heats obtained in the same way, the several groups exhibit very closely corresponding numbers, *e. g.* in the third group:—

	Molecular		Molecular	
	Volume.	Heat.	Volume.	Heat.
B.....	19	8·3	1·3	1·4
Al	13	9·4	1·9	1·3
Sc	18	10·4	1·4	1·2
Ga	17	9·8	1·5	1·2
Y	23	11·6	1·1	1·1
In	19	11·1	1·3	1·1
La	25	12·2	1·0	1·0

Although the last numbers are not absolutely identical in both columns, yet there can be no doubt that these relations are not due to mere chance. It is evident that the smallest molecular heat corresponds to the smallest molecular volume. Perhaps some relation may be found between these facts and Mendelejeff's hypothesis on the structure of molecules.

Hence it is clear that the relations between

$$\frac{\text{molecular volume}}{\text{molecular heat}} \text{ are approximately} = \text{const.}$$

The following examples show that we have $\frac{\text{vol.}}{\text{heat}} = \frac{5}{3}$, or the constant = 1·66; and it is strange that the variations are not larger with such different experimental material.

Cu_2O	1·26
BeO	1·34
(Li_2O	1·4)
MgO	1·54
CaO	1·80
ZnO	1·41
HgO	1·73
Be_2O_3	2·29
Al_2O_3	1·37
Sc_2O_3	1·69
Ga_2O_3	1·74
In_2O_3	1·73

Y_2O_3	1·74
La_2O_3	2·06
SiO_2	1·97
ZrO_2	1·59
SnO_2	1·60
CeO_2	1·69
ThO_2	1·85
MoO_3	1·73
Er_2O_3	2·09
Yb_2O_3	1·69
Di_2O_3	1·78 &c.

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(5) The same alteration of atomic heat is observed in the case of the oxygen contained in the oxides. Nilson has already pointed out that the oxygen combined with metals, having small atomic weight, also exhibits a small atomic heat. If we arrange the values so obtained, as far as the insufficient material allows, we get the following relations:—

	I.	II.	III.	IV.	V.	VI.
2.	(1·8) Li	2·4 Be	2·6 B			
3.	.....	Mg 3·8	Al 2·6	Si 2·8		
4.	.....	3·4 Ca	2·7 Sc	3·8 Ti		
5.	Cu 4·6	Zn 3·9	Ga 2·9			
6.	.....	.....	3·5 Y	3·6 Zr	.....	4·1 Mo
7.	.....	.....	In 3·1	Sn 3·8		
8.	.....	.....	4·0 La	4·4 Ce		
9.						
10.	.....	.....	.....	.....	.....	4·1 W
11.	.....	Hg 4·8				
12.	.. ...	.....	.....	4·0 Th		

Thus we see that the atomic heat of oxygen increases with the atomic weight, exhibiting a minimum in the upper corner on the left. Therefore we can calculate that value to be for lithia about 1·8.

Hence the numbers obtained by Nilson for the molecular heat of beryllia, and the atomic heat of the oxygen contained in it, serve to prove the dyad nature of beryllium and its position as II. —2 more satisfactorily than the opposite conclusion on the triad nature of the metal.

(6) But all the *chemical* reactions of beryllium compounds, some of them being different from those of the other dyad metals*, may be explained by the three equations following, from the periodic law:—

$$(a)\quad \mathrm{Li} : \mathrm{Be} = \mathrm{Be} : \mathrm{B},$$

$$(b)\quad \mathrm{Li} : \mathrm{Na} = \mathrm{Be} : \mathrm{Mg} = \mathrm{B} : \mathrm{Al},$$

$$(c)\quad \mathrm{Li} : \mathrm{Mg} = \mathrm{Be} : \mathrm{Al} = \mathrm{B} : \mathrm{Si}.$$

Beryllium is distinguished from the other metals of the second group in the same manner as lithium is from the alkaline metals, and as boron is from the triad elements. I will not enter further into this question, as it has been thoroughly treated by Mendelejeff in his papers†, and especially in his Russian 'Principles of Chemistry' (vol. ii. p. 826). If we

* See Nilson and Pettersson's paper, Wied. *Ann.* iv. p. 554.

† *Ann. Chem. Pharm.* Suppl. p. 8; Chem. News, xl. p. 303.

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regard beryllium as a triad, lithium must also be regarded as a dyad ($Li'' = 14$, oxide LiO) and as a member of the alkaline-earths group; for that metal forms, as is well known, a difficultly soluble phosphate and carbonate and no *stable* hydrosulphate or hydrocarbonate, its oxide combines only slowly and with a slight development of heat with water to form a non-deliquescent hydroxide, its nitrate and chloride are soluble in alcohol-ether and very deliquescent, the metal cannot be reduced by charcoal, &c.

But all those reactions, as well as the reactions of beryllium, are completely explained by the three above-mentioned equations of the position of lithium and beryllium.

(7) The circumstance that beryllium forms double fluorides finds its explanation in the feeble basic properties of beryllia.

(8) Also the easy volatilization of beryllium chloride may be easily deduced from the above equations (*b*) and (*c*) without it being necessary to assume the formula Be_2Cl_6 for it.

(9) The isomorphism of beryllium oxide with alumina is of no importance, as ZnO and ZrO_2 crystallize in the same forms. But also the salts of lithium are not isomorphous with those of sodium, any more than those of Be and Mg, or B and Al.

Repeating shortly what has been said, it follows that in all probability the atom of beryllium is $Be'' = 9{\cdot}1$, and its oxide BeO, from

(1) the molecular volume of beryllia,
(2) the molecular volume of its sulphate,
(3) the molecular heat of the earth,
(4) the atomic heat of the oxygen contained in it,
(5) from the chemical nature of beryllium.

The numbers which refer to this formula do not stand, as Messrs. Nilson and Pettersson would have us believe, without analogy in the whole domain of chemistry; on the contrary, they exactly correspond to the position of dyad beryllium in the periodic law. Moreover they find *a complete analogy in the element lithium*, so far as the physical properties of the compound of this metal have been investigated. They give a new confirmation of the view that beryllium forms, *like eleven other elements with a small atomic weight*, an exception to the law of Dulong and Petit.

Owens College, Manchester,
December 14, 1880.

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X. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. x. p. 449.]

December 1, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On Remains of a small Lizard from the Neocomian Rocks of the Island of Lesina, Dalmatia, preserved in the Geological Museum of the University of Vienna." By Prof. H. G. Seeley, F.R.S., F.G.S.

2. "On the Beds at Headon Hill and Colwell Bay in the Isle of Wight." By Messrs. H. Keeping and E. B. Tawney, M.A., F.G.S.

The authors criticised the views put forward by Prof. Judd in his paper published in the Q. J. G. S. xxxvi. p. 13, and supported those established by the late E. Forbes and the publications of the Geological Survey. At the west end of the island, viz. at Totland and Colwell Bays, the authors stated that there is only one marine series, the Middle Headon, which they traced continuously through the cliffs—identifying it bed by bed at various points—the result entirely corroborating the sections of the Geological Survey. The section at the N.E. end of Headon Hill was described in detail, and Prof. Judd's interpretation of this part of the section analyzed. Prof. Judd places the marine Middle Headon at this point at the level of the sea, maintaining that 250 feet (the altitude of the Bembridge limestone-quarry) of beds intervene between the Bembridge limestone and the sea-level. The authors maintained that the top of the marine series is about 105 feet above the sea-level, that thickness of beds intercalated above the Middle Headon having no existence in fact, also that the Brockenhurst bed does not exist below the Bembridge quarry, where it is supposed to be (concealed by gravel) by Prof. Judd, and stated that there is no gravel at that spot to conceal any thing, and that the beds which do exist there are the freshwater Osborne and Upper Headon beds as described by E. Forbes. They then adduced fossil evidence confirmatory of the stratigraphical; thus out of 57 species collected this summer at Colwell Bay they found 53 at Headon Hill. (2) The sections at Whitecliff Bay and the New Forest (Brockenhurst) were next described. At Whitecliff Bay the 90 feet of beds which constitute the Middle Headon of the Survey section have been renamed "Brockenhurst series" by Prof. Judd. The authors maintained that the Brockenhurst bed, identical as to its fossils and position with that of the Whitley Ridge cutting, is represented by the lower 2 feet only, immediately above the freshwater Lower Headon. The Middle Headon at Whitecliff Bay contains lower zones than any developed in the Middle

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Headon of Headon Hill; for the Brockenhurst bed is entirely absent from the west of the island. The authors maintained that Prof. Judd has assigned a false position to this bed in his vertical section of New-Forest beds, and that instead of being higher than the Venus-bed horizon, it is plainly below it, since at Whitley Ridge it lies on the Lower Headon, being succeeded by the Venus-bed and then by the Upper Headon. The palæontological evidence was then discussed; and it was objected to Prof. Judd's lists that he has mixed up the Colwell Bay and Brockenhurst fossils in one list, thereby begging the question. In opposition to his statistics the authors maintained that the Brockenhurst bed has about 48 per cent. of species which pass up from the Barton beds, while the Venus-bed series of either Colwell Bay or Headon Hill have only 29 per cent., suggestive of the lower position of the former.

Further examination of the lists of fossils prepared from an examination of the Edwards collection shows that the Colwell-Bay and Headon-Hill marine beds have thirteen times more species common to themselves alone than either of them have in common with the Brockenhurst bed. The palæontological evidence is therefore in accord with the stratigraphical; they both occupy a higher zone than the Brockenhurst bed, which, when developed, occupies the base of the Middle Headon. The authors therefore reject Prof. Judd's term Brockenhurst series, and revert to the classification and nomenclature of the Geological Survey.

December 15.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Constitution and History of Grits and Sandstones." By John Arthur Phillips, Esq., F.G.S.

In the first part of this paper the author described the microscopic and chemical structure of a large series of grits, sandstones, and, in some cases, quartzites, of various geological ages, noticing finally several sands of more or less recent date. The cementing material in the harder varieties is commonly to a large extent siliceous. The grains vary considerably in form and in the nature of their enclosures, cavities of various kinds and minute crystals of schorl or rutile not being rare. The author drew attention to the evidence of the deposition of secondary quartz upon the original grains, so as to continue its crystal structure, which sometimes exhibits externally a crystal form. This is frequently observable in sandstone of Carboniferous, Permian, and Triassic age. Felspar grains are not unfrequently present, with scales of mica and minute chlorite and epidote. Chemical analyses of some varieties were also given. The author then considered the effect of flowing water upon transported particles of sand or gravel. It results from his investigations that fragments of quartz or schorl less than $\frac{1}{50}$" in

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diameter retain their angularity for a very long period indeed, remaining, under ordinary circumstances, unrounded; but they are much more rapidly rounded by the action of wind. It is thus probable that rounded grains of this kind in some of the older rocks, as, for example, certain of the Triassic sandstones, may be the result of æolian action.

The Chair was then taken by J. W. Hulke, Esq., F.R.S., V.P.G.S.

2. "On a New Species of *Trigonia* from the Purbeck Beds of the Vale of Wardour." By R. Etheridge, Esq., F.R.S., President. With a Note on the Stratigraphical Position of the Fossil by the Rev. W. R. Andrews.

XI. *Intelligence and Miscellaneous Articles.*

NOTE ON MR. BROWNE'S PAPER "ON ACTION AT A DISTANCE." BY REV. A. J. C. ALLEN, B.A., FELLOW AND ASSISTANT TUTOR OF PETERHOUSE, CAMBRIDGE.

IN his paper in the Phil. Mag. for December 1880, Mr. Browne certainly appears to establish the proposition that we cannot explain the phenomena of cohesion &c. in a satisfactory way by means of the direct impact of bodies; and then he goes on to deduce from this that action at a distance (a very small distance indeed, but still finite) must exist in these cases, and therefore may be equally well allowed in the case of bodies at a great distance, as the earth and sun. Now it appears to me that in reality very much is gained in the scientific explanation of phenomena when they are transferred from the region of action at great distances to action at distances such as intervene between the particles of solid bodies. I cannot explain this better than by quoting the words of Clerk Maxwell, who has himself done so much in the direction of getting rid of action at a distance by his explanation of electrical and magnetic phenomena by means of stress in the medium which fills the electrical or magnetic field.

"When we see one body acting on another at a distance, before we assume that the one acts directly on the other we generally inquire whether there is any material connexion between the two bodies; and if we find strings or rods, or framework of any kind capable of accounting for the observed action between the bodies, we prefer to explain the action by means of the intermediate connexions, rather than admit the notion of direct action at a distance.

"Thus, when two particles are connected by a straight or curved rod, the action between the particles is always along the line joining them; but we account for this action by means of a system of internal forces in the substance of the rod. The existence of these internal forces is deduced entirely from observation of the effect of external forces on the rod; and the internal forces themselves are

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generally assumed to be the resultants of forces which act between particles of the rod. Thus the observed action between two distant particles is in this instance removed from the class of direct action at a distance by referring it to the intervention of the rod; the action of the rod is explained by the existence of internal forces in its substance; and the internal forces are explained by means of forces assumed to act between the particles of which the rod is composed—that is, between bodies at a distance which, though small, must be finite.

"The observed action at a considerable distance is therefore explained by a great number of forces acting between bodies at very small distances, for which we are as little able to account as for the action at any distance, however great.

"Nevertheless the consideration of the phenomenon, as explained in this way, leads us to investigate the properties of the rod, and to form a theory of elasticity which we should have overlooked if we had been satisfied with the explanation by action at a distance."*

But even granting that such an explanation as this is no help at all towards getting rid of action at a distance, we are not limited to direct impact as the only possible explanation. If we conceive beings living in a continuous fluid the existence of which they were not capable of perceiving, two vortex rings in the fluid would to them appear to act on one another directly at a distance, and yet there is no real action at a distance, and no impact of hard bodies on one another. Is it not possible that in the future the actions of the particles of bodies on one another may be explained in some such way as this? or even that the phenomena of direct impact themselves may be so accounted for?

A CONTRIBUTION TO THE THEORY OF SO-CALLED ELECTRICAL EXPANSION OR ELECTROSTRICTION. BY PROF. L. BOLTZMANN, OF GRAZ.

Let the interval between two concentric conducting spherical shells with radii a and $a+\alpha$ be filled with a solid elastic dielectric of which the constant of dielectricity is D. Let its interior be charged with the potential p, and its outer surface connected to earth. If we use the notation employed in Lamé's *Leçons* to the elastic-traction force

$$R_1=\lambda\theta+2\mu\frac{dU}{dr}$$

the expression

$$\frac{kp^2D^2a^4}{8\pi r^4\alpha^2},$$

in which

$$k=\frac{(D-\frac{1}{2})(D-1)}{D^2},$$

is then added.

* 'Electricity and Magnetism,' vol. i. p. 122.

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If the conducting shells are destitute of rigidity, and firmly connected with the dielectric, upon each superficial unit of its inner surface the pressure $\frac{p^2D^2(1-k)}{8\pi a^2}$ acts, and upon the same of its outer surface

$$\frac{p^2D^2(1-k)(a+\alpha)^4}{8\pi a^4\alpha^2}.$$

The dilatation of volume effected by these forces, referred to the unit of volume, is

$$\frac{\Delta v}{v} = \frac{3p^2D^2[2\lambda+2\mu-k(\lambda+2\mu)]}{16\pi\mu(3\lambda+2\mu)\alpha^2}.$$

If the dielectric fills in the same manner the space between two coaxial cylindrical surfaces of radii a and $a+\alpha$, to the elastic force R_1 there is added $\frac{kp^2a^2D^2}{8\pi r^2\alpha^2}$, and the pressure-forces

$$\frac{p^2D^2(1-k)}{8\pi a^2} \text{ and } \frac{p^2D^2(1-k)(a+\alpha)^2}{8\pi a^2\alpha^2}$$

act upon the unit of the inner and of the outer surface. The elongation thereby produced of the unit of length of the hollow cylinder is

$$\frac{\Delta l}{l} = \frac{p^2k\lambda D^2}{16\pi\mu(3\lambda+2\mu)\alpha^2}.$$

Therefore, for $D=\infty$, $\frac{\Delta v}{v} = 3\frac{\Delta l}{l}$; and for a lower value of D, $\frac{\Delta v}{v} > 3\frac{\Delta l}{l}$. The former case could be approximately realized for glass. Both quantities are independent of r, directly proportional to p^2, inversely to α^2. According to this, Röntgen's view may be justified, that Quincke's observations form at least no refutation of the explanation of electrical expansion from the ordinary forces of electric action at a distance and elasticity. An ordinary Franklin's plate of the thickness α must then behave (for instance, even in regard to its optical properties) as if it were compressed by the force $\frac{p^2D^2}{8\pi\alpha^2}$ upon the superficial unit. After this the construction of the general equations for the deformation of a dielectric body by electrification, or of a magnetic body by magnetization, presents no further difficulty.—*Kaiserl. Akad. der Wissensch. in Wien*, 1880, no. xxiii., *Sitz. math.-naturw. Classe*, Nov. 4, 1880.

ON THE ABSORPTION OF SOLAR RADIATION BY THE CARBONIC ACID OF OUR ATMOSPHERE. BY DR. ERNST LECHER.

Experiments having already been made by Magnus and Tyndall,

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according to which a pretty considerable absorbing-power for radiant heat is possessed by carbonic acid, the author has attempted, in the above investigation, to extend those observations, and to apply them to the atmospheric absorption of the solar radiation. This view is at variance with the generally accepted assumption that the aqueous vapour of our atmosphere is the true absorbent of the sun's radiation—an assumption the groundlessness of which the author has demonstrated in another place.

First, the radiation of a gas-lamp provided with a glass cylinder was investigated; and it was found that carbonic acid in a length of

214	536	705	917	917 millims.

transmitted

94·8	93·8	91·4	90	89 per cent.

of the incident radiation.

Further experiments (which, for the sake of the pure air, were made out of Vienna, at Greifenstein) show that the solar radiation also suffers a considerable weakening on passing through carbonic acid gas. A layer of this gas of one metre thickness absorbs about 13 per cent. when the sun's altitude is 59°; this number lessens in the same proportion as the sun descends. This proves that the absorption of the solar radiation by carbonic acid is a selective absorption, and that the thicker the atmospheric layer through which the rays have already passed, the scarcer do the absorbable wave-lengths become. The author calculates from his experiments the proportion of carbonic acid contained in the atmosphere, and finds it to be 3·27 parts by volume in 10,000, which number agrees so well with the data given elsewhere, and based upon chemical analysis, that there is the best prospect of determining in this way the carbonic-acid content of the atmosphere, together with its variations and changes, accurately and also at heights where direct measurements are impossible.—*Kaiserliche Akademie der Wissenschaften in Wien, Sitz. d. math.-naturw. Classe*, Nov. 4, 1880.

SPECTRAL REACTION OF CHLORINE AND BROMINE.

BY M. LECOQ DE BOISBAUDRAN.

When, by the old process, the spectrum of a small bead of chloride of barium is observed in a gas-flame, the lines proper to that compound soon disappear and give place to those of the oxide. Yet for a long time some chlorine remains in the baryta thus heated; the destruction of the $BaCl_2$ is only completed after its volatilization. So, on drawing the induction-spark upon the small calcined mass, the nebulous lines proper to $BaCl_2$ are very distinctly seen, as well as the narrow barium-lines.

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In seeking for traces of chlorine or bromine, I operate thus:—

A vertical platinum wire (about $\frac{3}{4}$ of a millim. in diameter) is bent up at its lower part in the form of a hook or a U. Upon this half-ring I fuse, at a bright-red heat, 0·001–0·002 gramme of pure carbonate of baryta; then I place in the bend of the wire a little drop of the liquid which is to be examined*, evaporate to dryness, and even, *for a brief moment*, push the heat to incipient redness. The at least partial fusion of the mass is advantageous, by permitting it to spread like a varnish at the surface of the platinum wire.

After cooling, a second platinum wire (of $\frac{3}{4}$ to 1 millim. diameter) is brought quite close (at 1 or $1\frac{1}{2}$ millim. distance) below the bend of the first, *a little forward* towards the slit of the spectroscope. The induction-spark, not condensed, then gives a spectrum in which are seen the lines of $BaCl_2$ or $BaBr_2$. One three-thousandth of a milligramme of chlorine or bromine can thus be recognized. No doubt, with some precautions, even this degree of sensitiveness could be surpassed, especially for chlorine.

If in such a sample the quantity of chlorine is somewhat considerable, the wire can be heated to dazzling whiteness for several minutes without detriment to the lines of $BaCl_2$, of which it is often very difficult to get rid by prolonged calcination of the platinum wire.

The resistance of $BaBr_2$ to heat appears to be notably less.—*Comptes Rendus de l'Académie des Sciences*, Dec. 6, 1880, t. xci. pp. 902, 903.

ON RADIOPHONY. BY M. E. MERCADIER.

The phenomenon which I call *radiophony* is that recently discovered by Mr. G. Bell, in which a radiation (such as constitutes a solar ray) rendered intermittent according to a determined period, on falling upon bodies cut into plates, produces a sound of the same period. With a view to a possible application of this phenomenon to optic telegraphy, I have been obliged to study it closely, and have obtained results of which the following are the principal:—

I. *Radiophony does not appear to be an effect produced by the mass of the receiving plate vibrating transversely in its entirety as an ordinary vibrating plate.* Indeed any plate whatever, in the conditions in which the phenomenon is produced, (1) reproduces *equally well* all the successive sounds from the deepest possible to shrill sounds which, in my experiments, went up to 600 or 700 double vibrations

* If the material contains nitrates, it is expedient to expel NHO_3 by SH_2O_4, then this by pure BaO. If there is much sulphuric acid or sulphate, this is likewise got rid of by BaO.

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per second, and that *without solution of continuity* ; (2) reproduces *equally well accords* in all possible tones, varying, if it be wished, *in a continuous manner* on the velocity of the apparatus which produces the intermittences being varied in a continuous manner. For this purpose the apparatus is composed of a glass wheel, to the surface of which a paper disk carrying four series of apertures, in number 80, 60, 50, 40, is pasted. This permits the *successive* sounds of a perfect accord to be produced by passing the luminous ray into the apertures of a series and removing the support of the wheel itself, and perfect superposed *accords* to be produced by leaving the support of the wheel immovable and concentrating, with a cylindrical lens, the light upon the four series of apertures at the same time. Now no rigid vibrating plate known is susceptible of producing such effects. (3) Moreover the sounds produced do not change, either in timbre or height, with the thickness and width of the receiving plates; they do not even sensibly change in intensity with the breadth or even with the thickness, in transparent plates like glass and mica, between wide limits, which for glass in particular extend from 0·5 millim. to 2 or 3 centims. thickness. This permitted me to employ receiving plates of 1 square centim., particularly tourmaline plates of that dimension*. (4) A cracked, split plate of glass, copper, aluminium, &c., produces very sensibly the same effects as when it is intact.

II. *The nature of the molecules of the receiver, and their mode of aggregation, do not appear to exercise a predominant part in influencing the nature of the sounds produced.* In fact, (1) with equal thickness and surface, the receivers, whatever their nature may be, produce sounds of the same height. (2) When the thickness of the receiving plates diminishes more and more, the specific differences existing between their modes of producing the phenomenon become more and more attenuated when their surface exposed to the radiations is made identical—for instance, by covering them all with a pellicle of lampblack. (3) The effect produced by ordinary radiations is, *cæteris paribus*, very nearly the same for transparent substances as different as glass, mica, Iceland spar, gypsum, quartz cut parallel or perpendicular to the axis. It is the same when polarized radiations are employed—for example, by means of a Nicol.

III. *Radiophonic sounds certainly result from the direct action of the radiations upon the receivers*; for (1) the intensity of the phenomenon is gradually diminished by diminishing the quantity of radiations received, by aid of diaphragms of variable aperture. (2) The radiations being polarized, and a thin analyzer, such as a plate of tourmaline, being taken for the receiving plate, the sounds produced

* Besides, for opaque plates, the thinner they are, the greater is the intensity of the sounds; copper, aluminium, platinum, and especially zinc foil, of $\frac{1}{20}$ of a millim. for example, gives excellent results.

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present the variations of intensity corresponding to those of the radiation itself when the polarizer or analyzer is rotated.

IV. *The phenomenon seems to result chiefly from an action upon the surface of the receiver*; for its intensity greatly depends upon the nature of the surface. Every operation that diminishes the reflecting-power and augments the absorptivity of the surface influences the phenomenon; roughened, tarnished, oxidated surfaces are the most suitable.

The intensity of the phenomenon is considerably augmented when the surface is coated with certain black substances, in powder or not, such as bitumen of Judea, platinum-black, and especially lampblack; but this effect is not particularly sensible unless the coated plates are very thin: thus, if the thickness be from about $\frac{1}{10}$ to $\frac{2}{10}$ of a millimetre, remarkable effects are obtained. I have consequently constructed very sensitive radiophonic receivers with the aid of very thin plates of zinc, glass, mica, smoked. The employment of these sensitive receivers has permitted me to arrive at the following result:—

V. *Radiophonic effects are relatively very intense.* In fact, I can actually obtain them not only with the radiations of the sun and of an electric lamp, but with the oxyhydrogen-light, the flame of an ordinary gas-burner, and consequently with the radiations from intermediate sources, such as petroleum-lamps, a platinum spiral heated to redness by a Bunsen burner, &c.

VI. *Radiophonic effects appear to be produced chiefly by the radiations of great wave-length*, called *calorific*. In order to demonstrate this, without stopping for the moment at the employment of cells filled with absorbent liquids such as alum, iodine dissolved in sulphide of carbon, &c., the effect of which cannot be very pure, I tried to explore with a sensitive receiver the spread-out spectrum of the acting radiations. I succeeded in doing so with the electric light of 50 Bunsen elements and by employing ordinary lenses and a prism of glass. I ascertained that the maximum effect is produced by the red and invisible infra-red radiations; starting from the yellow and proceeding to the violet and beyond, under the conditions under which I worked I did not obtain any sensible effect. The experiment has at different times been realized with receivers of smoked glass, platinum-blacked platinum, and zinc with its surface bare.

I think I ought to signalize the preceding facts, which have appeared to me certain. Many points still remain for me to signalize and elucidate; this shall be the aim of an early subsequent communication.—*Comptes Rendus de l'Académie des Sciences*, Dec. 6, 1880, t. xci. pp. 929–931.

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THE

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JOURNAL OF SCIENCE.

[FIFTH SERIES.]

FEBRUARY 1881.

XII. *On the Measuring of Electrical Conductivities.* By G. KIRCHHOFF*.

FOR the comparison of the resistances of short wires Sir W. Thomson† has given a method founded on an arrangement which is a modification of the Wheatstone bridge. Another method which may serve for the same purpose is in many respects more convenient, and, as it appears, not inferior in accuracy; it is based on the employment of a differential galvanometer, the windings of which can be set so that a current flowing successively through them produces no deflection of the needle. If a circuit be formed out of the two resistances to be compared, and a battery, and the two wires of the differential galvanometer be inserted as secondary closings to them, and if the resistance of one of these wires be varied till deflection of the needle vanishes, then the ratio of the resistances to be compared will be equal to the ratio of the resistances of the galvanometer-wires, provided that the above-indicated setting has been given to the windings. If now such resistances be added to the galvanometer-wires that deflection of the needle again vanishes, the ratio of the added resistances will also be equal to the ratio of those to be compared ‡.

* Translated from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*, July 1880, pp. 601–613.

† Phil. Mag. [4] xxiv. p. 149 (1862).

‡ Tait (Trans. R. Soc. Edinb. vol. xxviii. 1877–78, p. 737) has com-

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If from the resistance of a wire (determined by one or the other of the methods mentioned) we wish to ascertain its conductivity, and an error of the order of the ratio of the thickness of the wire to its length is inadmissible, the currents at the place of junction of three branches of the conducting system must not be regarded as linear; therefore the theory of current-distribution in non-linear conductors must be employed.

Of the resistance of a non-linear conductor (taken strictly) one can only speak on the assumption that the part of its surface through which electricity flows consists of two surfaces, within each of which the potential is constant. The difference of the values of the potential in these two *electrode surfaces* (as they may be called), divided by the amount of electricity which flows through the one or the other in the unit of time, is then a constant of the conductor, which is even called its resistance. We must here take into view a more complicated case—namely, that instead of two electrode surfaces more of them are present, of which, however, each is again a surface of equal potential.

Let n be the number of electrode-surfaces, $P_1, P_2, \ldots P_n$ the potential-values in them, and $J_1, J_2, \ldots J_n$ the quantities of electricity which flow through them in unit time into the conductor. If these intensities, between which the relation

$$J_1+J_2+\ldots+J_n=0$$

must subsist, be given, the quantities P up to an additive constant are determined; namely, calling this c,

$$\begin{aligned}
P_1&=c+a_{11}J_1+a_{12}J_2+\ldots+a_{1n}J_n,\\
P_2&=c+a_{21}J_1+a_{22}J_2+\ldots+a_{2n}J_n,\\
&\ldots\ldots\ldots\ldots\\
P_n&=c+a_{n1}J_1+a_{n2}J_2+\ldots+a_{nn}J_n,
\end{aligned}$$

where the quantities a designate constants of the conductor—constants which, however, by the way, are not independent of one another, but can be reduced to $\frac{n(n-1)}{2}$ reciprocally independent quantities.

Let it now be assumed that $n=4$, that electrode surfaces 1 and 4 are connected to the poles of a battery, and surfaces 2 and 3 to the ends of a wire (one of the wires of a differential

pared a similar method with Thomson's and found it superior to the latter; the differential galvanometer used by him, however, was not so arranged that the windings could be displaced; and in consequence of this he had to forego some essential advantages offered by the method recommended in the text.

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galvanometer). Let the resistance of this wire be w. Then

$$J_3 = -J_2, \quad J_4 = -J_1.$$

Further, we have, on one hand,

$$P_2 - P_3 = wJ_2,$$

and, on the other,

$$P_2 - P_3 = (a_{21} - a_{31} - a_{24} + a_{34})J_1 + (a_{22} - a_{32} - a_{23} + a_{33})J_2.$$

Putting

$$a_{21} - a_{31} - a_{24} + a_{34} = \rho,$$

$$a_{22} - a_{32} - a_{23} + a_{33} = r,$$

we hence get

$$\rho J_1 = (w - r)J_2.$$

The quantity ρ can be qualified as the value which $P_2 - P_3$ has in the case that $J_2 = -J_3 = 0$, and $J_1 = -J_4 = 1$. If the conductor is a very long thin wire and the surfaces 1 and 2 are situated quite close to one end, surfaces 3 and 4 to the other, ρ is the resistance of the conductor; with another form of the conductor, ρ may be named *one* resistance of it.

Let us now imagine a second conductor, besides the one above-mentioned, possessing also the properties attributed to that. To the quantities ρ and r in that, P and R in this may correspond. Electrode surfaces 2 and 3 of the second conductor are to be connected to the ends of the second wire of the differential galvanometer, whose first wire touches with its ends the surfaces 2 and 3 of the first conductor; electrode surfaces 1 and 4 of the second conductor are to communicate respectively with the electrode surfaces 4 and 1 of the first, the one by a wire, the other by a galvanic series. An arrangement is then produced such as is described at the beginning of this communication. In this arrangement J_1 has the same value for both conductors; and the same holds also for J_2 when the needle of the galvanometer shows no deflection and this instrument possesses the construction presupposed. Hence, if W is the resistance of the second wire of the galvanometer, we have

$$PJ_1 = (W - R)J_2,$$

and therefore

$$P(w - r) = \rho(W - R).$$

Now if w' and W' are two other values of the resistances of the two galvanometer-wires at which the needle likewise suffers no deflection, then in like manner is

$$P(w' - r) = \rho(W' - R);$$

therefore also

$$P(w' - w) = \rho(W' - W).$$

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If the resistance ρ can be theoretically expressed by the conductivity and the dimensions of the conductor in question, if these dimensions have been measured, and if P and the ratio of the resistances $w'-w$ and $W'-W$ are known, that conductivity can be calculated.

An essential basis of the above-instituted considerations was the assumption that the electrode surfaces are surfaces of equal potential. An electrode surface having this property can be found if electricity be brought to the conductor through a surface of which the dimensions are infinitesimal compared with all the dimensions of the conductor. If, namely, around a point in this surface we describe a sphere with a radius indefinitely large compared with its dimensions, but indefinitely small compared with the dimensions of the conductor, the part of this sphere which is within the conductor is a surface of equal potential; and hence, if it be reckoned as part of the boundary of the conductor considered, it is an electrode surface of the sort supposed. Such a surface can be found in another way, if the conductor, entirely or in part, consists of a cylinder of any form we please of cross section, the length of which considerably exceeds the dimensions of the cross section, and if the electricity flows in at its end. A cross section distant a moderate multiple of the longest chord of the cross section from this end can then be regarded as a surface of equal potential, and therefore also as an electrode surface of the kind in question, if it be considered as belonging to the boundary of the conductor.

An arrangement which can, accordingly, be employed when the conductivity of a substance is to be measured which is present in the form of a cylinder of moderate length, is the following:—The current of the battery is conducted into and out of the rod through its ends; the ends of one of the wires of the galvanometer are in conducting connexion with spikes which are pressed against its enveloping surface in two points whose distances from the nearest end amount to a moderate multiple of the longest diameter of the cross section: Two cross sections of the rod can then be regarded as electrode surfaces 1 and 4, and two segments of spherical surface described with infinitesimal radii about the two spikes as electrode surfaces 2 and 3. The resistance ρ is then equal to the distance of the cross sections passing through the two spikes, divided by their surface and the conductivity.

It may, however, be desirable to make use of the whole length of the given rod, in order to make the resistance to be measured as large as possible. If the rod has the shape of a rectangular parallelepipedon, then the arrangement can be

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recommended in which, of the four angles of a long side-face, two adjacent to a long edge are connected with the battery, and the two others with the galvanometer-wire; the electrode surfaces 1, 2, 3, 4 are then the octants of four infinitesimal-sphere surfaces whose centres are in the four angles mentioned. This method is very convenient in execution, and is also interesting inasmuch as it is an application of the beautiful theory of the propagation of a current in a rectangular parallelepipedon.

Mr. Greenhill* has already, for the potential in a point of a rectangular parallelepipedon, to which electricity flows through one point and is withdrawn through a second, constructed an expression which can here be taken for a starting-point. Let the point of an angle of the parallelepipedon be the origin of coordinates, let the edges proceeding from it be the axes of coordinates, a, b, c the lengths of the edges, x_1, y_1, z_1 the coordinates of the positive, x_4, y_4, z_4 the coordinates of the negative electrode; further, let the intensity of the current be $=1$, and k be the conductivity of the parallelepiped; then the potential ϕ in reference to the point (x, y, z) is

$$= \frac{1}{32abck}\int_0^\infty dt(\mathrm{F}_1 - \mathrm{F}_4),$$

where

$$\mathrm{F}_1 = \left(\theta_3\left(\frac{x-x_1}{2a}, \frac{i\pi t}{4a^2}\right) + \theta_3\left(\frac{x+x_1}{2a}, \frac{i\pi t}{4a^2}\right)\right)$$
$$\times \left(\theta_3\left(\frac{y-y_1}{2b}, \frac{i\pi t}{4b^2}\right) + \theta_3\left(\frac{y+y_1}{2b}, \frac{i\pi t}{4b^2}\right)\right)$$
$$\times \left(\theta_3\left(\frac{z-z_1}{2c}, \frac{i\pi t}{4c^2}\right) + \theta_3\left(\frac{z+z_1}{2c}, \frac{i\pi t}{4c^2}\right)\right);$$

F_4 results from F_1 when the index 4 is substituted for the index 1, and

$$\theta_3(w, \tau) = \Sigma e^{\nu(2w+\nu\tau)\pi i},$$

the sum being taken so that all the whole numbers from $-\infty$ to $+\infty$ are put for ν.

By employing the partial differential equation which is satisfied by the θ functions it can be demonstrated, in the way marked out by Mr. Greenhill, that the hereby defined function ϕ satisfies the partial differential equation which it ought to satisfy; it can further be shown that the boundary-conditions and the conditions of constancy are fulfilled which hold for ϕ,

* Proc. Camb. Phil. Soc., Oct. to Dec. 1879, p. 293.

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and thus proved that the potential in question must, to within an additive constant, be equal to the expression adduced.

In order to obtain the value of ϕ which corresponds to the arrangement described above, let us put

$$x_1=0, \quad y_1=0, \quad z_1=0.$$

$$x_4=0, \quad y_4=0, \quad z_4=c.$$

Taking advantage of the circumstance that

$$\theta_3(w\pm\tfrac{1}{2},\tau)=\Sigma(-1)^\nu e^{\nu(2w+\nu\tau)\pi i}=\theta_0(w,\tau),$$

we get

$$F_1-F_4=8\theta_3\Big(\frac{x}{2a},\frac{i\pi t}{4a^2}\Big)\theta_3\Big(\frac{y}{2b},\frac{i\pi t}{4b^2}\Big)\Big(\theta_3\Big(\frac{z}{2c},\frac{i\pi t}{4c^2}\Big)-\theta_0\Big(\frac{z}{2c},\frac{i\pi t}{4c^2}\Big)\Big),$$

or, since

$$\theta_3(w,\tau)-\theta_0(w,\tau)=2\theta_2(2w,4\tau),$$

$$\phi=\frac{1}{2abck}\int_0^\infty\theta_3\Big(\frac{x}{2a},\frac{i\pi t}{4a^2}\Big)\theta_3\Big(\frac{y}{2b},\frac{i\pi t}{4b^2}\Big)\theta_2\Big(\frac{z}{c},\frac{i\pi t}{c^2}\Big)dt.$$

To find the resistance denoted by ρ, we have to form the difference of the values taken by this expression for

$$x=a, \quad y=0, \quad z=0,$$

and for

$$x=a, \quad y=0, \quad z=c,$$

provided that b is the length of that edge which is perpendicular to the surface of the four angles used as electrodes.

Taking into consideration that

$$\theta_2(w+1,\tau)=-\theta_2(w,\tau),$$

and writing, for the sake of shortness,

$$\theta(\tau) \text{ for } \theta(0,\tau),$$

we get

$$\rho=\frac{1}{abck}\int_0^\infty dt\,\theta_0\Big(\frac{i\pi t}{4a^2}\Big)\theta_3\Big(\frac{i\pi t}{4b^2}\Big)\theta_2\Big(\frac{i\pi t}{c^2}\Big).$$

The numerical calculation of this integral becomes proportionally easy if we, by inserting an intermediate boundary, divide it into two, and at suitable places introduce, instead of the θ functions with the modulus τ, the θ functions with the modulus $-\frac{1}{\tau}$. Since

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$$\theta_0\left(\frac{i\pi t}{4a^2}\right)=\frac{2a}{\sqrt{\pi}}\frac{1}{\sqrt{t}}\theta_2\left(\frac{4a^2i}{\pi t}\right),$$

$$\theta_3\left(\frac{i\pi t}{4b^2}\right)=\frac{2b}{\sqrt{\pi}}\frac{1}{\sqrt{t}}\theta_3\left(\frac{4b^2i}{4t}\right),$$

$$\theta_2\left(\frac{i\pi t}{c^2}\right)=\frac{c}{\sqrt{\pi}}\frac{1}{\sqrt{t}}\theta_0\left(\frac{c^2i}{\pi t}\right),$$

we can put

$$\rho=\frac{4}{k\pi^{\frac{3}{2}}}\int_0^\lambda\frac{dt}{t^{\frac{3}{2}}}\theta_2\left(\frac{4a^2i}{\pi t}\right)\theta_3\left(\frac{4b^2i}{\pi t}\right)\theta_0\left(\frac{c^2i}{\pi t}\right)$$
$$+\frac{1}{abk\sqrt{\pi}}\int_\lambda^\infty\frac{dt}{\sqrt{t}}\theta_0\left(\frac{i\pi t}{4a^2}\right)\theta_3\left(\frac{i\pi t}{4b^2}\right)\theta_0\left(\frac{c^2i}{\pi t}\right),$$

where λ is a positive quantity which can be disposed of at pleasure. The first of these two parts of ρ can be written

$$\frac{8}{k\pi^{\frac{3}{2}}}\int_{\frac{1}{\sqrt{\lambda}}}^\infty dt\theta_2\left(\frac{4a^2t^2i}{\pi}\right)\theta_3\left(\frac{4b^2t^2i}{\pi}\right)\theta_0\left(\frac{c^2t^2i}{\pi}\right),$$

or, since

$$\theta_3(\tau)=\Sigma e^{\nu^2\tau\pi i},$$
$$\theta_2(\tau)=\Sigma e^{(\nu+\frac{1}{2})^2\tau\pi i},$$
$$\theta_0(\tau)=\Sigma(-1)^\nu e^{\nu^2\tau\pi i},$$

$$\frac{8}{k\pi^{\frac{3}{2}}}\Sigma(-1)^n\int_{\frac{1}{\sqrt{\lambda}}}^\infty dte^{-((2l+1)^2a^2+4m^2b^2+n^2c^2)t^2},$$

where the sum is to be taken thus: for each of the symbols l, m, n all the whole numbers from $-\infty$ to $+\infty$ are to be put. Now let us suppose

$$\int_x^\infty dte^{-t^2}=\mathrm{U}(x);$$

and consequently, if α denotes a positive quantity,

$$\int_x^\infty dte^{-\alpha^2t^2}=\frac{1}{\alpha}\mathrm{U}(\alpha x);$$

for these functions $\mathrm{U}(x)$, and for the interval from $x=0$ to $x=3$, Kramp, as is known, has calculated a table; for higher values of the argument their values are found with the aid of the semiconvergent series

$$\mathrm{U}(x)=\frac{e^{-x^2}}{2}\left(\frac{1}{x}-\frac{1}{2}\frac{1}{x^3}+\frac{1}{2}\cdot\frac{3}{2}\frac{1}{x^5}-\ldots\right).$$

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Putting, for further abbreviation,

$$\frac{1}{x}\mathrm{U}(x)=f(x),$$

the first part of ρ becomes

$$\frac{8}{k\pi^{\frac{1}{2}}}\frac{1}{\sqrt{\lambda}}\Sigma(-1)^n f\left(\sqrt{\frac{(2l+1)^2a^2+4m^2b^2+n^2c^2}{\lambda}}\right).$$

The second portion can be written

$$\frac{2}{abk\sqrt{\pi}}\int_{\sqrt{\lambda}}^{\infty}dt\theta_0\left(\frac{i\pi t^2}{4a^2}\right)\theta_3\left(\frac{i\pi t^2}{4b^2}\right)\theta_0\left(\frac{c^2i}{\pi t^2}\right),$$

or

$$\frac{2}{abk\sqrt{\pi}}\Sigma(-1)^l\int_{\sqrt{\lambda}}^{\infty}dte^{-\left(\frac{l^2}{a^2}+\frac{m^2}{b^2}\right)\frac{\pi^2}{4}t^2}\theta_0\left(\frac{c^2i}{\pi t^2}\right),$$

where, in taking the sum in reference to l and m, all the numbers from $-\infty$ to $+\infty$ are to be put for these symbols. To calculate the member of this sum which corresponds to determined values of l and m, let us make

$$\left(\frac{l^2}{a^2}+\frac{m^2}{b^2}\right)\frac{\pi^2}{4}=\beta^2,$$

with the condition that β is positive; the term then becomes

$$\frac{2}{abk\sqrt{\pi}}(-1)^l\Sigma(-1)^n\int_{\sqrt{\lambda}}^{\infty}dte^{-\beta^2t^2-\frac{n^2c^2}{t^2}},$$

where, in the summation, all numbers from $-\infty$ to $+\infty$ are to be put for n, or

$$\frac{1}{abk\sqrt{\pi}}\frac{(-1)^l}{\beta}\Sigma(-1)^n\left\{e^{2n\beta c}\mathrm{U}\left(\beta\sqrt{\lambda}+\frac{nc}{\sqrt{\lambda}}\right)+e^{-2n\beta c}\mathrm{U}\left(\beta\sqrt{\lambda}-\frac{nc}{\sqrt{\lambda}}\right)\right\}.$$

For the case that $\beta=0$, and that consequently l and m are simultaneously $=0$, this result does not hold good; the term corresponding to these values of l and m is

$$\frac{2}{abk\sqrt{\pi}}\int_{\sqrt{\lambda}}^{\infty}dt\theta_0\left(\frac{c^2i}{\pi t^2}\right),$$

or

$$\frac{2}{abk\sqrt{\pi}}\int_{\sqrt{\lambda}}^{\infty}dt\Sigma_n(-1)^n e^{-\frac{n^2c^2}{t^2}}.$$

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Since

$$\int dt e^{-\frac{\gamma^2}{t^2}} = te^{-\frac{\gamma^2}{t^2}} - 2\gamma \mathrm{U}\left(\frac{\gamma}{t}\right) + \text{const.},$$

this expression is

$$= \mathrm{C} + \frac{2}{abk\sqrt{\pi}} \Sigma(-1)^n \left(2nc\,\mathrm{U}\left(\frac{nc}{\sqrt{\lambda}}\right) - \sqrt{\lambda} e^{-\frac{n^2c^2}{\lambda}}\right),$$

in which C denotes a quantity independent of λ, and for n its absolute value may constantly be put. The value of C is ascertained when the same term is calculated after replacing

$$\theta_0\left(\frac{c^2 i}{\pi t^2}\right) \text{ by } \frac{\sqrt{\pi}}{c} t\theta_2\left(\frac{i\pi t^2}{c^2}\right);$$

it thereby becomes

$$\frac{1}{abck} \int_\lambda^\infty dt \theta_2\left(\frac{i\pi t}{c^2}\right),$$

or

$$\frac{c}{abk} \frac{4}{\pi^2} \Sigma \frac{1}{(2\nu+1)^2} e^{-(2\nu+1)^2 \frac{\pi^2 \lambda}{4c^2}}.$$

By putting $\lambda = 0$ and remembering that

$$\Sigma \frac{1}{(2\nu+1)^2} = \frac{\pi^2}{4}$$

we get

$$\mathrm{C} = \frac{c}{abk}.$$

Of surpassing importance for the investigation is the case that c is to be regarded as infinitely great, a, b, l as finite. In this case, of the terms whose sum constitutes the value of ρ, all vanish in which n has a value different from 0, and we get

$$\rho = \frac{c}{abk} = \frac{2\sqrt{\lambda}}{abk\sqrt{\pi}} + \frac{2\sqrt{\lambda}}{abk\sqrt{\pi}} \Sigma(-1)^l f\left(\frac{\pi}{2}\sqrt{\lambda}\sqrt{\frac{l^2}{a^2} + \frac{m^2}{b^2}}\right)$$
$$+ \frac{8}{k\pi^{\frac{3}{2}}\sqrt{\lambda}} \Sigma f\left(\sqrt{\frac{(2l+1)^2a^2 + 4m^2b^2}{\lambda}}\right),$$

where, in taking the sum, all the whole numbers from $-\infty$ to $+\infty$ are put for l and m.

If we make

$$\lambda = \frac{2ab}{\pi},$$

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the result is

$$abk\,.\,\rho = c - \frac{\sqrt{8ab}}{\pi}\left\{1 - \Sigma(-1)^l f\left(\sqrt{\frac{\pi}{2}}\sqrt{l^2\frac{b}{a} + m^2\frac{a}{b}}\right) - 2\Sigma f\left(\sqrt{\frac{\pi}{2}}\sqrt{(2l+1)^2\frac{b}{a} + 4m^2\frac{a}{b}}\right)\right\},$$

or

$$abk\,.\,\rho = c - \frac{8\sqrt{2ab}}{\pi}\left\{\tfrac{1}{4} - \sum_0^\infty\sum_0^\infty \epsilon f\left(\sqrt{\frac{\pi}{2}}\sqrt{l^2\frac{b}{a} + m^2\frac{a}{b}}\right)\right\},$$

where

$$\epsilon = 0 \text{ if } l = 0 \text{ and } m = 0,$$

$$\epsilon = \tfrac{1}{2} \text{ if } l = 0 \text{ or } m = 0,$$

$$\epsilon = -1 \text{ if } l \text{ and } m \text{ are uneven},$$

$$\epsilon = +1 \text{ in all other cases.}$$

If

$$b = a,$$

it follows that

$$a^2k\,.\,\rho = c - a\,.\,0{\cdot}7272.$$

In deducing this result it is sufficient to calculate twice the sum of four terms.

The derivation of the given expression for ρ rests upon the assumption that the ratios $\frac{c}{a}$ and $\frac{c}{b}$ can be regarded as infinite; in fact, however, very moderate values of these ratios suffice to make that expression very nearly correct. It is so even in the case that

$$a = b = \frac{c}{2}.$$

In this case the value of ρ is particularly easily ascertained. According to one of the equations set up, ρ is then

$$\rho = \frac{1}{a^3k}\int_0^\infty dt\,\theta_0\theta_3\theta_2,$$

where the modulus τ is the same for all three θ functions, namely $\frac{i\pi t}{4a^2}$. Now we have, as is well known,

$$\theta_0\theta_3\theta_2 = \frac{1}{\pi}\theta'_1 = \Sigma(-1)^\nu(2\nu+1)e^{-\frac{(2\nu+1)^2\pi^2 t}{16a^2}};$$

and hence follows

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$$\rho = \frac{8}{ak\pi^2}\Sigma(-1)^\nu \frac{1}{2\nu+1}$$

$$= \frac{16}{ak\pi^2}(1-\tfrac{1}{3}+\tfrac{1}{5}-\ldots.)$$

$$= \frac{4}{ak\pi}, \text{ i. e. } = \frac{1}{ak}\,.\,1{\cdot}2732.$$

But if we calculate ρ for this case from the previously derived formula, it is found little different from this, namely

$$= \frac{1}{ak}\,.\,1{\cdot}2728.$$

XIII. *A Theory of the Constitution of the Sun, founded upon Spectroscopic Observations, original and other.* By CHARLES S. HASTINGS*.

FRAUNHOFER discovered the lines in the solar spectrum, known by his name, in 1814. Many efforts to determine their origin followed. One of the most ingenious and carefully considered was that of Professor Forbes, in 1836†. He concluded that, if their origin is in the solar atmosphere, the light from the limb must exhibit stronger lines than that from the centre. His method was to examine the spectrum before and during an annular eclipse; as he found no recognizable change, his deduction was "that the sun's atmosphere has nothing to do with the production of this singular phenomenon."

The point was again touched upon by Sir David Brewster and Dr. Gladstone in a joint study of the spectral lines, published in 1860‡. Here "each of the authors came independently to the conclusion that there is no perceptible difference in this respect between the light from the edge and that from the centre of the solar disk."

In 1867 Ångström§ repeated the experiment with negative results. Lockyer's‖ efforts also, in 1869, were attended with no better results.

In 1873, four years later, I devised and made an apparatus by which a perfect juxtaposition of the spectra of the centre and limb was secured. This apparatus and certain of the

* From Silliman's American Journal of Science, January 1881.

† "Notes relative to the supposed Origin of the Deficient Rays in the Solar Spectrum," Phil. Trans. 1836, pp. 453–456.

‡ "On the Lines of the Solar Spectrum," Phil. Trans. 1860, pp. 149–161.

§ Phil. Mag. 1867, xxxiii. p. 76. ‖ Proc. Roy. Soc. vol. xvii. p. 350.

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results gained by its use were described in a note, "On a comparison of the spectra of the limb and the centre of the Sun," published in Silliman's Journal, vol. v. (1873), pp. 369–371. I was then a student at Yale College, and soon after left New Haven, when the research was necessarily interrupted. I hoped, however, that the novelty and interest of the observations might lead others, possessed of the necessary apparatus, to develop the results of this method of investigation. But as nothing as been published on this subject since that time, I was glad to have an opportunity to continue the investigation in the summers of 1879 and 1880. The results of my labour are embodied in this paper.

The method adopted in the recent observations is exactly the same as that described in the article cited; instead, however, of the equatorial of the Sheffield Scientific School, I used a Clark equatorial of 9·4 in. aperture and 120 in. focal length, which was kindly placed at my disposal by the gentlemen in Hartford, to whom it belongs*. The New-Haven spectroscope, too, of twelve effective prisms, was replaced by one of which the dispersing member was a Rutherfurd grating on speculum-metal, either of 8648 or 17,296 lines to the inch at will. These gratings were of the largest size, having a ruled surface of about $1\frac{3}{4}$ inch square.

The immediate results I give in order of refrangibility of the lines observed, as no observed variations in them can be attributed to any thing other than the temporary modifications of transparency in our atmosphere. The numbers are the places on Ångström's maps as nearly as could be ascertained without micrometer.

Line (C) 6561·8 is cleaner and wider at limb; *i. e.* the haze on either side of the line as ordinarily seen is much reduced.

6431 is slightly stronger at centre than at limb.

6371 is visible at centre but not at limb.

(D_1) 5894·8 slightly less hazy at limb.

(D_2) 5889·0 decidedly cleaner at limb.

A fine line very close to its more refrangible side is either wanting or much fainter in spectrum of limb.

5577·5 is much stronger at limb.

5440 ± (not on Ångström's chart) is a little stronger at limb.

The Mg lines 5183·0, 5172·0, 5166·5 ($b_1 b_2 b_4$) are cleaner at limb. The line b_3, belonging to a different element, does not show such a peculiarity.

5045 (a faint line not in Å.) is stronger at limb.

* My acknowledgments for this courtesy are gratefully accorded to Mr. Edgecomb, its former owner, and to Mr. Howard and Mr. Chapin, its present owners.

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4919±, a faint line slightly stronger at limb.
(F) 4860·6 is much cleaner, more free from haze, at limb.
4702·3 seems cleaner at limb.
4340·0 cleaner at limb.
4226·4 shows less haze at limb.
4101·2 is a very hazy line, so represented by Ångström; but at limb it is practically free from haze—a striking difference.
4045 is slightly less hazy at limb.

Other differences have been recorded; but only these have been observed more than once each.

Any theory of the sun, worthy of attention, must not only explain the above-described phenomena, but also others better known, and as yet not accounted for satisfactorily. Of these the most noteworthy is the spectroscopic appearance of a spot and its penumbra. As is well known, such a spectrum exhibits a very strong general absorption, with a very slightly modified elective absorption. A few faint lines appear in the spot-spectrum which are not otherwise seen; and a few faint lines of the ordinary spectrum are strengthened. A careful examination has persuaded me that the spectrum of a spot differs from that of the unbroken photosphere just as the spectrum of the limb differs from that of the centre of the disk, save that the variations are more pronounced. Indeed I could have considerably extended the list of lines strengthened at limb by an examination of the spot-spectrum, where the variations appeal to the eye more clearly.

The accepted theory of the spots attributes the phenomenon to the absorption of the solar light by cooler denser gases of the same nature as those producing the Fraunhofer lines. Familiar experiments teach, however, that as the density of a gas increases, the change in the character of its radiation is shown in its spectrum by the broadening of its distinctive spectral lines, which at the same time grow more ill-defined. Therefore it follows that, according to the law connecting radiation and absorption, dark lines produced by such a gas must also, under similar conditions, show increased breadth and diminished sharpness. That no such changes are to be recognized is a fatal objection to the theory.

Another class of unexplained phenomena is the duplicity of certain lines of the solar spectrum, lines which are single in the spectra of terrestrial sources. Of these, Prof. Young has discovered E_1, b_3, and b_4, with others.

My own observations can be arranged very simply in classes, and will then better lend themselves to theoretical discussion.

I. The most important fact of all is that the differences in the two spectra of centre and limb are extremely minute,

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escaping all but the most perfect instruments and all methods which do not place them in close juxtaposition.

II. Certain lines, the thickest and darkest in the spectrum, notably those of hydrogen, magnesium, and sodium, which appear with haze on either side, in the spectrum of the centre of the solar disk, are deprived of this accompaniment in that of the limb.

III. Certain very fine lines (four observed) are stronger at limb.

IV. Other very fine lines (two or three observed) are stronger at centre.

The ordinarily accepted theory of the origin of the Fraunhofer lines fails to explain the phenomena as observed. That is, if we suppose the photosphere, whether solid, liquid, gaseous, or cloud-like, to yield a continuous spectrum which is modified only by the selective absorption of a surrounding atmosphere, then the absorption must be greater at the limb than at the centre of the solar disk; and this must be true independently of the thickness of that atmosphere, as well as of the form, rough or otherwise, of the surface of the photosphere. This evident consequence, pointed out in the first place by Forbes nearly half a century ago, cannot be avoided. There is but one way of maintaining the theory and escaping Forbes's conclusion already quoted, and that the course pursued by Kirchhoff in the original statement of his theory of the solar constitution*—namely, by assuming that the depth of the reversing atmosphere is not small compared with the radius of the sun. But innumerable observations during the score of years which have lapsed since that time prove that such a reversing atmosphere must be very thin. The famous observation of Professor Young during the total eclipse of 1870, when he saw appreciably all the Fraunhofer lines reversed, has naturally been received as the strongest confirmation of Kirchhoff's views as to the locus of the origin of the dark lines. But this very observation restricts the effective atmosphere (save for hydrogen and one or two other substances) to a depth of not more than 2″. Thus, singularly enough, the very observation which led to the firmest belief among spectroscopists in the correctness of Kirchhoff's view, exposed at the same time its most vulnerable point.

Another theory of the solar constitution, that of Faye, assigns a different seat to the stratum producing the Fraunhofer lines, namely the photosphere itself. Regarding the principal radiation of the sun as coming from solid or liquid particles floating in a gaseous medium, the cloud-like stratum

* *Untersuchungen über das Sonnenspectrum*, Berlin, 1862, pp. 14, 15.

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thus formed is necessarily somewhat transparent. According to his views, these particles are the sources of the continuous spectrum, and the medium in which they float is the locus of the selective absorption*. Thus he attempts to reconcile the general theory of Kirchhoff with the observations and deductions of Forbes, which, as we have seen, were a constant stumblingblock in the way of accepting Kirchhoff's explanation.

Lockyer seems to have accepted this theory, and to have defended it in the earlier portion of his work†; but in 1872, after Young's important observation of 1870 and its confirmation in 1871, he changed his views, and regarded the layer just outside the photosphere as the true seat of the selective absorption producing the Fraunhofer lines‡. I supposed in 1873 that my observations then published could be explained on Faye's hypothesis.

There is, however, a fatal objection to the explanation as given by this theory. If the luminous particles are precipitated from the vapours of the photosphere, they cannot be at a higher temperature than the circumambient gases; on the contrary, on account of their greater radiating power they must be slightly cooler. But the fundamental theory of absorption demands a lower temperature for the vapour producing dark lines than that of the principal source of light behind it; consequently this view of Faye cannot be accepted without great modifications.

Before advancing any theory of my own, it may be well to emphasize two principles taught by the theory of absorption, to which all hypotheses must be conformable. That Faye's fails in this is sufficient cause for its rejection.

1st. To produce dark lines in a spectrum by absorption, the source of absorbed light *must* be at a higher temperature than that of the absorbing medium.

2nd. There is an inferior limit of brightness below which the course of absorbed light cannot go without the spectral lines becoming bright.

Of these, the first is familiar and requires here neither proof nor comment; the second, though not less evident, is less familiar because less important. As we shall make use of it, however, it may be well to enforce it by reference to common experience. Were it not true, it would be impossible to see

* *Comptes Rendus*, vol. lx. 1865.

† See "A Lecture delivered at the Royal Institution, May 28th, 1869," quoted in Lockyer's 'Solar Physics,' pp. 220, 221; also 'The Rede Lecture,' May 24th, 1871, quoted in 'Solar Physics,' pp. 317, 318.

‡ See revised report of two lectures delivered at Newcastle-upon-Tyne in October 1872, 'Solar Physics,' p. 409.

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bright lines in the spectrum of any flame to which daylight had access; for in this case the conditions demanded by the first principle are fully met, the sun being the origin of the daylight. That we do not see absorption-lines is due, then, alone to the lack of necessary brilliancy in the daylight.

Thus much premised, we can frame a theory which explains all the observed phenomena exhibited by the spectroscope, and is also rendered highly probable by the revelations of the telescope.

As is well known, the solar surface, when examined with a powerful telescope of large aperture, presents a granulated appearance, the granules in general subtending an angle of a fraction of a second only. Probably this appearance is better known to the majority of astronomers by means of Professor Langley's admirable drawings* rather than by personal observation. These granules I regard as marking the locus of currents directed generally from the centre of the sun. About these currents are necessarily currents in an opposite direction which serve to maintain a general equilibrium in the distribution of mass. Let us consider the action of such an ascending current. Starting from a low level at a temperature which we may regard as above the vaporizing-point of all elements contained in it, as it rises to higher levels it cools, partly by radiation, more by expansion, until finally the temperature falls to the boiling-point of one or more of the substances present. Here such substances are precipitated in the form of a cloud of fine particles which are carried on suspended in the current. The change of state marked by the precipitation is accompanied by a sudden increase in radiating-power; hence these particles rapidly lose a portion of their heat and become relatively dark, to remain so until they are returned to lower levels by the currents in a reverse direction.

In this theory, it will be observed, there is nothing which does violence to our accepted notions of the solar constitution. Indeed it differs chiefly from that of Faye in localizing the phenomena of precipitation, instead of regarding it as proper to all portions of the photosphere, and (what is quite as important) in supposing the precipitation confined to one or two elements only. I shall attempt to define these elements further on.

In our theory, then, the granules are those portions of upward currents where precipitation is most active, while the darker portions between these bodies are where the cooler products of this change with accompanying vapours are sinking to lower levels.

* Silliman's Journal, vol. vii. 1874, and vol. ix. 1875, plates.

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Having stated the theory, we will now apply it to the four classes of phenomena defined above.

From the nature of the condensation, the granules or cloudy masses must be very transparent, because the condensation is confined to elements which have very high boiling-points, and because such elements can be but a portion, perhaps but a small portion, of the whole matter contained in the upward currents.

It is not *à priori* improbable that we receive light from many hundreds of miles below the general outer surface of the photosphere. Since these cloud-like sources of intenser radiation are surrounded on all sides by descending currents of colder vapours, all the white light which comes to us must have passed through media capable of modifying it by selective absorption. Again, since at the centre of the solar disk we can see as far into the photosphere as at the limb and practically no further, the phenomena of absorption ought to be, on the whole, the same in both regions.

Thus the fundamental and most important class of phenomena above classified finds a simple and logical explanation.

With regard to the phenomena of class II., we have but to define the problem in order to find the solution at hand. All the lines of class II. belong to vapours which lie high in the solar atmosphere, as is evident from their frequent reversal in the chromosphere. On the centre of the disk these lines are hazy or "winged," but not so at the limb. To the spectroscopist this aspect is characteristic of greater pressure, that is of more frequent molecular impact. The observation, then, proves that the dark lines of hydrogen, magnesium, sodium, &c., as seen at the centre of the solar disk, are produced by the elements in question at a higher pressure than the corresponding lines at the limb. Accepting our theory, this must be so; for supposing the transparency of the photosphere is such that we can see into it a distance of 2000 miles, then at the centre of the disk we have light modified by selective absorption all the way from the extreme outer chromosphere down to 2000 miles below the upper level of the photosphere; while 10″ from the limb the light, though coming from the same depth of vapour measured along the line of vision, has its lowest origin more than 1700 miles further from the sun's centre than in the previous case. Of course the numbers here used have no definite significance; but modify them as we will within the bounds of probability, the reasoning remains the same.

Suppose now a certain vapour which is confined to the upper stratum of the photosphere, or rather one of which the lower limit is thus restricted; then, according to the reason-

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ing of Forbes, the force of which has been shown, its absorption-lines ought to be strongest at the limb. This is the condition which produces the phenomena of Class III.

Before discussing the final class we must recall a fact familiar to the most casual observer of the sun, namely, that lying upon the photosphere is a stratum producing a very strong general absorption, so strong indeed that the disk is probably less than a fourth as brilliant near the edge as at the centre. This layer is *very thin*, as proved by the great difference in brilliancy between the upper and lower portions of faculæ. Since the difference of absorption at the two levels is very great, the conclusion follows, because the facula itself is so low that it rarely, if ever, appears as a projection on the limb of the sun. For convenience, let us call this layer A.

Imagine, then, a stratum of vapours B above the layer just described, which are not represented at all in the photosphere, and which are of nearly the same temperature as this layer A*. Then (for the sake of simplicity regarding this layer as having no selective absorption) suppose all beneath the two spherical shells in consideration to be removed. In the spectroscope, light from such a source as the two layers A and B would yield a continuous spectrum; for the inner shell (A), radiating only white light, would be robbed of nothing not supplied in equal quantity by radiation from the outer shell (B), since they are of the same temperature. If such layers as these really do exist about the sun, we can now readily state the appearances which would be presented by a sun so constituted, if the threefold system should be studied spectroscopically. In the centre of the projected disk, the lines proper to the exterior shell (B) would be reversed, *i. e.* dark. As we approached the edge, however, *owing to the opacity of the inner shell*, the conditions would approximate to what they would be if the layers A and B existed alone, the central body being removed, and the lines would fade; if faint, they would vanish. This is our explanation of the phenomena of Class IV.

Every theory involves certain conditions. We finally judge

* This supposition is not opposed to probability; for though we must regard the temperature generally decreasing in passing from the photosphere outward, it does not follow that this decrease is continuous. A similar general law may be stated for our own atmosphere; but in a clear night the air in the immediate vicinity of the ground is colder than that just above. The explanation of this phenomenon is familiar in the theory of dew and hoar frost. Analogous causes for irregularity in the distribution of temperature in the solar atmosphere must be even more efficacious, since the layer A is probably a more vigorous radiator than the earth, and the gases above it are certainly far more diathermanous than our atmosphere.

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of the soundness or unsoundness of any theory largely from the consideration of these implied conditions and of the extent to which they are fulfilled by it. For instance, our explanation of the fact that certain very fine lines are stronger at the centre (IV.) demands that the substances giving such lines should be found in the chromosphere, indeed mainly restricted to the chromosphere. Fortunately I can say that one of them (6371), which I first discovered and measured carefully, is identical with the 14th line of Young's second Catalogue of Chromosphere Lines. The other one, the wave-length of which I took from Ångström's chart without correction, may correspond with Young's 9th (6429·9) line of the same catalogue, which differs in place by only one sixth the distance between the D lines. This I shall test at the earliest opportunity.

If the theory I have proposed is correct, it affords the first definite evidence of the existence of *chemical compounds* in the sun; for in accordance with it the lines of Class III. and Class IV. belong to substances which are not found in the lower photosphere. We know, however, that all gases must increase in density in passing from their outer limit toward the centre of the sun; and we have seen a proof of this in the case of hydrogen and certain other vapours in the discussion of our observations, which showed that the characteristic lines indicated greater density when they originated at greater depths. The only escape from the contradiction is in the assumption that the lines of the last two cases (III. & IV.) are due to compound vapours having a dissociation-temperature below that of the lower photosphere. Of course the substances of Class IV. have a lower dissociation-temperature than those of Class III.

A naturally suggested and legitimate subject of speculation is as to the nature of the substance which, by precipitation, forms the cloud-masses of the photosphere. We may predicate three properties with greater or less positiveness, viz.:—

1st. The substance has a boiling-point above that of iron; for iron-vapour at a lower temperature exists in its immediate neighbourhood.

2nd. The molecular weight is probably not great; for though precipitated below the upper natural limit of its vapour, there are few elements found in abundance above it, and those in general of low vapour-density.

3rd. The element is not a rare one. Of these guides the last is of the least value.

The substances which apparently meet all these conditions are carbon and silicon; nor is it easy to name any other which will. Accepting for a moment as an hypothesis that the light

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coming from the sun is radiated by solid or liquid particles of carbon just at the point of vaporization, let us see if the facts of observation fulfil the implied conditions.

As a first consequence, we see that the temperature and light of the photosphere are defined as those of solid carbon at the point of volatilization. In the electric arc there is a very small area of the positive carbon-pole at this high temperature. Though this area is in a very disadvantageous position for observation, and can consequently have but a disproportionally small share in producing the total effect, the splendour of the electric light might almost tempt us to believe the guess a valid one. Another consequence implied, however (namely, that the spectral lines proper to simple carbon are absent in the solar spectrum), is doubtless better adapted as a crucial test of the hypothesis than a study of the electric light. There has been evidence recently offered that carbon-lines are present in the solar spectrum. Granting this, we perceive that the photosphere contains solid or liquid particles hotter than carbon vapour, and consequently not carbon.

I am then inclined to suspect that the photospheric material may be silicon, which, though denser in the gaseous state than carbon, is not improbably more abundant. There is also good reason to suppose that carbon is precipitated at a higher level; and the analogous, but less common, element boron may add a minor effect.

In the explanations which I shall give of the remaining phenomena, it may serve to fix the ideas to think of the granules which characterize the sun's photosphere as clouds of a substance like precipitated silicon. At any rate we are sure that the substance in question, so far as we know it, has properties similar to those of the carbon group.

I have given plausible explanations of all the phenomena included specially in my own observations. It remains to discuss the others, briefly mentioned above.

The substance precipitated cools very rapidly, as it is an excellent radiator, separated from space only by extremely diathermanous media. It forms, then, a smoke-like envelope which ought to exert just such a general absorption as that observed at the limb of the sun. It is thin, because of the relatively great density of the substance in the liquid or solid state; thus the apparent brilliancy of the faculæ is readily understood.

If there is any disturbing cause which would tend to direct currents of gas, over a considerable area of the solar surface, toward a point, this smoke, instead of quietly settling down to lower levels between the granules, would concentrate about

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this point, there exercising a marked general absorption which would betray itself as a spot. At this place the suspended particles would sink to lower levels with constantly increasing temperature, until finally, heated to intense incandescence, they would revolatilize. Thus the floor or substratum of every spot must be a portion, depressed it is true, of the photosphere. All the spectroscopic phenomena of spots, which have proved so perplexing, are thus naturally and easily explained.

In the immediate neighbourhood of a spot the centripetal currents bend down the ordinary convection or granule-producing currents so that they are approximately level. Before, the latter cooled suddenly by rarefaction in their upward course; now they cool mainly by the much slower process of radiation: thus, while before the locus of precipitation was restricted, it is now greatly extended. This is the cause of the great elongation of the granules in the penumbra, a real elongation, I imagine, and not merely an apparent one.

Finally, concerning the close duplicity of certain lines, we may reason thus:—If we could surround the sun by a stratum of gas hotter than the photosphere and much rarer than that producing the corresponding Fraunhofer lines, we should, as is shown by a course of reasoning which I have given in another place*, see each dark line divided by a sharp bright line in its centre—that is, doubled. But as a consequence of the theory, this supposed condition must be practically met in the case of certain vapours in the sun. The gases just over the granules, in the vertical currents, are at a very high temperature, essentially that of the condensing material itself, consequently much hotter and rarer than the relatively low-lying vapours which, as we have seen, produce the Fraunhofer lines.

There are, however, certain evident limitations to these conditions; in other words, we cannot expect to see all the dark lines doubled by any increase of dispersive power. For instance, a line must have a marked tendency to broaden with increased pressure; otherwise the duplication cannot be pronounced. Again, the layer of rare vapour must be thin, or its temperature cannot be relatively high throughout, as demanded by the theory. This evident condition doubtless gives the reason why the hydrogen-lines, though the broadest in the solar spectrum, are not sensibly double.

The theory of the constitution of the sun above proposed may be briefly recapitulated thus:—

Convection currents, directed generally from the centre of the sun, start from a lower level, where the temperature is probably above the vaporizing temperature of every substance.

* "On Lockyer's Hypothesis," Amer. Journ. Chem. vol. i. p. 15.

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As these currents move upward, they are cooled, mainly by expansion, until a certain element (probably of the carbon group) is precipitated. This precipitation (restricted, from the nature of the action) forms the well-known granules. There is nothing which has come under my observation which would indicate a columnar form in these granules under ordinary circumstances.

The precipitated material rapidly cools, on account of its great radiating-power, and forms a fog or smoke, which settles slowly through the spaces between the granules till revolatilized below. It is this smoke which produces the general absorption at the limb and the "rice-grain" structure of the photosphere.

When any disturbance tends to increase a downward convection current, there is a rush of vapours at the outer surface of the photosphere toward this point. These horizontal currents or winds carry with them the cooled products of precipitation, which, accumulating above, dissolve slowly below in sinking. This body of "smoke" forms the solar spot.

The upward convection currents in the region of the spots are bent horizontally by the centripetal winds. Yielding their heat now by the relatively slow process of radiation, the *loci* of precipitation are much elongated, thus giving the region immediately surrounding a spot the characteristic radial structure of the penumbra.

This conception of the nature of the penumbra implies a ready interpretation of a remarkable phenomenon amply attested by the most skilful observers, and, as far as my knowledge goes, wholly unexplained—namely, the brightening of the inner edge of the penumbra in every well-developed spot*.

This interpretation is perhaps most readily imparted by a comparison of the hot convection currents in the two cases. When the convection current is rising vertically, the medium is cooled by expansion until the precipitation-temperature is reached, when all the conductible material appears *suddenly*, save as it is somewhat retarded by the heat liberated in the act. Immediately afterward the particles become relatively dark by radiation. In the horizontal current a very different condition of things obtains. Here the medium does not cool dynamically by expansion, but only by radiation—hence, since the radiation of the solid particles is enormously greater than that of the supporting gas, practically by that of the particles

* Relating to this phenomenon, see important observations by Professor Langley, Silliman's Journal, vol. ix. (1875) p. 194; also *Le Soleil*, par Le P. A. Secchi, Paris, 1875, chap. iv. p. 80, and particularly fig. 46, p. 90, with explanatory text.

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themselves. Thus, after the first particle appears, it must remain at its brightest incandescence until all the material of which it is composed is precipitated. From this we see that such a horizontal current must increase gradually in brilliancy to its maximum, and then suddenly diminish—an exact accordance with the facts as observed.

Johns Hopkins University,
Baltimore, Sept. 1880.

XIV. *On* Professor Osborne Reynolds's *Paper "On certain Dimensional Properties of Matter in the Gaseous State."* *By* GEORGE FRANCIS FITZGERALD, *M.A., F.T.C.D.**

THE Royal Society has published in its Philosophical Transactions, 1879, part ii., a valuable and interesting paper by Prof. Osborne Reynolds, "On certain Dimensional Properties of Matter in the Gaseous State." In it he first calls attention to the phenomenon of "Thermal Transpiration," and professes to give not only explanations of this and allied phenomena, but also the means of calculating their amounts. The late Prof. J. Clerk Maxwell expressed his obligation to Prof. Reynolds for having directed his attention to this phenomenon, and to the necessity of investigating the conditions at the surface of separation of a solid and a gas. At the same time he expresses himself dissatisfied with Prof. Reynolds's investigation, although he does not point out what parts he considers faulty. As Prof. Reynolds's paper is very elaborate, and necessarily somewhat difficult, not only from the nature of the subject, but also, in parts, owing to the inelegant method that Prof. Reynolds has pursued, it may be of interest to the readers of the Philosophical Magazine to have some objections to his investigation pointed out, in order that Prof. Reynolds may have an opportunity of refuting them, and also in order that the doubts that Maxwell has raised as to the real value of the investigation may be shown either not to apply, or at least not to affect the essence of the results.

In the first place I must express my entire admiration for the first part of the paper, dealing with experiments. These were conducted both carefully and scientifically. They are not of that haphazard character so much in vogue with unscientific experimenters. Each is framed for the purpose of illustrating, verifying, or investigating some definite theory. Maxwell's objections, then, can only have referred to Part II. (Theoretical), beginning at § 53. Against this paragraph I desire to enter a protest, because it seems to assert that some other theories of the action of the radiometer widely different

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from those set forth in this paper have had supporters, and this I believe to be erroneous. Against § 54 I must likewise protest; for it is there asserted that with unlimited plates, "in order to produce results similar to those produced with limited plates, the gas between the two plates must maintain a greater steady pressure on the plate H" (the hot one) "than that which it exerts on the colder plate C." I wish Prof. Reynolds had mentioned what result with any plates could possibly be explained by such an absurd supposition. If he means that such a supposition would be necessary to explain a repulsion between the plates and between them and the sides of the containing vessel, he has not sufficiently considered the problem. I cannot believe that he is unable to see that if the gas between the plates is in a state of stress, such as he himself afterwards shows it might be, in which the pressure of the gas on each plate is the same, but greater than the pressure on a plane at right angles to the plates—if, I say, this were the state of the gas, it is evident that the pressure on the cold backs of the plates might be this less pressure, and so less than that on their opposed surfaces, and they would tend to recede from one another. Whether the gas between the plates would actually be in this state is another question; but to suppose it such is not absurd in the same manner as to suppose action and reaction unequal though opposite. It is hardly worth while pointing out that his whole illustration by means of batteries is beside this question, and that the tendency of the plates to separate is proportional to their area.

In § 60 Prof. Reynolds acknowledges that stresses in the gas, which turn out subsequently to be different pressures in different directions, cause the phenomena he is investigating, although in a letter to 'Nature' (Jan. 17, 1878, p. 220) he denied that such a condition of stress can have any thing to do with the cause of Mr. Crookes's results. It is this change in his views that makes his theory practically the same as the one he was then combating. With reference to the remaining paragraphs of this section I have nothing but congratulations to offer, the introduction of the "mean range" being in every way admirable and a notable advance in the methods of treating the subject. The assumption he makes as to the character of the motions of the molecules is, like the analogous one of Clausius, not only probable in itself, but justified in its results, and, from these, seems sufficient as a first approximation.

With the symbols and notation I have no fault to find; but I must enter a protest against his elaborate and totally unnecessary division of space into eight regions. He might have perfectly well calculated equations (43) to (47) with-

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out rendering a difficult subject tenfold as elaborate as was necessary. For instance, using Prof. Reynolds's notation, in calculating the mass carried across unit area perpendicular to x, his assumption, as far as the first approximation goes, comes to the same as supposing that the components of the velocity of a molecule going in the direction whose direction-cosines are $l : m : n$, are

$$u = lq + \mathrm{U}, \quad v = mq + \mathrm{V}, \quad w = nq + \mathrm{W};$$

so that

$$lq = \xi, \quad mq = \eta, \quad nq = \zeta,$$

and

$$q^2 = \xi^2 + \eta^2 + \zeta^2,$$

and

$$d\xi\, \partial\eta\, \partial\zeta = q^2 dq\, dl\, d\phi,$$

where

$$m = \sqrt{1 - l^2} \sin\phi$$

and

$$n = \sqrt{1 - l^2} \cos\phi.$$

From these we get that

$$\sigma'_x \mathrm{M} = \int_0^\infty \int_{-1}^{1} \int_0^{2\pi} \frac{\rho e^{-\frac{q^2}{a^2}}}{a^3 \pi^{\frac{3}{2}}} (lq + \mathrm{U}) q^2 dq\, dl\, d\phi$$
$$= \rho \mathrm{U},$$

and

$$s\Sigma \left(l \frac{d}{dx} + m \frac{d}{dy} + n \frac{d}{dz} \right) \delta\sigma'_x \mathrm{M}$$
$$= \frac{s'}{\sqrt{\pi}} \cdot \frac{d\rho a}{dx}.$$

As the molecules going in the $u+$ direction come from $-dx$ &c., this leads to the same result as Prof. Reynolds obtains, namely

$$\sigma_x \mathrm{M} = \rho \mathrm{U} - \frac{s'}{\sqrt{\pi}} \cdot \frac{d\rho a}{dx}.$$

This is really the same process as his, only that his takes up several pages and is very complicated, owing to his dividing his space into those eight regions and then even performing his integrations at different places several pages apart instead of all at once.

Now I come to a point that I hope Prof. Reynolds will be kind enough to explain; for I am sure he must have considered it, and that there must be some reason for what he does, though he gives none and, as far as I can see, does not notice

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the anomaly I want him to explain. It is that, after neglecting both s^2 and U^2 as small quantities, he nevertheless keeps in sU all through his equations and carries it forward into his results. I thought at first that this must have been owing to the terms he omitted not being of the same order in the result as those in sU; but I worked out some of them, and they seem to me to lead to terms of exactly the same order as those he has retained. Omitting these terms does not seem to alter the form of the result; yet it would explain Maxwell's doubts as to Prof. Reynolds's method being a good one for determining the amount of the result. I shall return to this presently, as I think it may explain a difference between his results and those of Clausius.

Having arrived at his fundamental equations, the way in which he treats them and takes into account the discontinuity at the surface of a solid is admirable; and the comparison of his results with experiment justifies his fundamental assumptions as far as this order of approximation is concerned. In this connexion, however, it is well to bear in mind that an assumption sufficient to explain one result may not explain another. For instance, a very great change might be made in Clausius's hypothesis without affecting the conduction of heat, but nevertheless very materially affecting the resultant stresses. I have already pointed out this in a paper, "On the Mechanical Theory of Crookes's Force" (Transactions of the Royal Dublin Society, 1878, p. 62, and Phil. Mag. Jan. 1879). In this paper I do not profess to have done more than point out how, upon particular assumptions as to the average motions of the molecules of the gas, these stresses may be most conveniently calculated.

When Prof. Reynolds comes to the question of Thermal Impulsion, I do not know whether to be more puzzled at what he retains in his equations or what he omits to notice as their result. Before considering the discontinuity near solids, he depends wholly upon the term in sU that I have already noticed as of the same order as several terms he has rejected. This must, I am sure, be explicable; and in any case the phenomena, as Maxwell asserts, may depend entirely upon the want of continuity at the surface of solids; and in the part of the investigation where this is introduced he treats the whole question *ab initio* and escapes this difficulty. I come next to the point that puzzles me by his omitting to notice it. I am sure, however, that he has done so from want of space and not from any oversight. The point is, that the pressure on a plane drawn in one direction in the gas is generally different from

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that on one drawn in another direction at the same point. It is this difference that Maxwell uses to measure the stress at a point. Prof. Reynolds always measures it by the difference of pressure on two parallel planes drawn at different points. For instance, on p. 830 equation (131) is

$$\frac{p_x - p_0}{p_1} = \frac{4}{\pi} \cdot \frac{s^3}{\alpha} \cdot \frac{d^2\alpha}{dx^2};$$

and $p_x - p_0$ was got by integrating $\frac{dp}{dx}$, and must consequently be the difference of pressures on two planes both perpendicular to x at two different points. Notwithstanding this, Prof. Reynolds proceeds to compare his with Maxwell's measure of the stress, and seems to talk of them as the same. That both measures of the stress may exist there is no doubt; but it is equally certain that Maxwell's might exist though Prof. Reynolds's vanished. For instance, in the case I have already mentioned at the beginning of this paper, of gas between two parallel plane solids, the pressure would of course be the same on all planes parallel to them, and so Prof. Reynolds's measure of the stress would vanish; while the pressure on a plane perpendicular to the solids might be different from that on them, and so Maxwell's measure of the stress would not vanish. As they ought to be both equal in the case Prof. Reynolds considers, I must suppose that he did not think it worth while noticing the difference, though his way of treating the matter would lead one to suppose, what I can hardly believe, that he never thought of their being any difference. Both measures of the stress must exist when the surfaces of equal pressure are curved. Maxwell's theory requires that this should be the case in order for there to be any stress. Contrary to what he seems to assert, Prof. Reynolds's equations do not give this result. They give, when the heat is passing between two infinite parallel planes,

$$p_x - p_y = -\frac{4H^2}{p\alpha^2},$$

where H is the quantity of heat passing; and this, though with an opposite sign, gives exactly the same law for the stress as has long ago been shown to follow from Clausius's hypothesis. I do not attribute much importance to the difference of sign; for if all the terms in s^3 are included that seem to me ought to be included, Prof. Reynolds's equations confirm Clausius's. That both measures of the stress exist when the surfaces of equal pressure are curved, is rendered obvious by considering the equilibrium of a shell of gas included between two concen-

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tric hemispheres of radii c_1 and c_2. If P_1 be the pressure on the inner one of radius c_1, and P_2 on the other, and if p be the mean pressure on the circular annulus of thickness c_2-c_1 between them, and be consequently the pressure at right angles to P_1 or P_2, then for the equilibrium of the shell we have

$$P_1c_1^2+p(c_2^2-c_1^2)=P_2c_2^2.$$

Hence

$$P_1-P_2=(P_2-p)\frac{c_2^2-c_1}{c_1^2}.$$

Now P_1-P_2 is Prof. Reynolds's measure of the stress, while P_2-p would be Maxwell's at the outer surface, while P_1-p would be his measure for the stress at the inner surface; and this latter is the same as Prof. Reynolds's when $c_2=\infty$, which is the case he compares. Where the discontinuity at the surface of the solid is taken into account, the equation Prof. Reynolds gives is

$$\frac{P_2-P_1}{P}=\tfrac{1}{2}\cdot\frac{c_2^2-c_1^2}{c_2^2}\cdot\frac{\alpha_c-\alpha'}{\alpha'}.$$

How this result is obtained is not very easy to see; for it does not seem to follow without additional assumptions to those he mentions; but, being derived *ab initio*, it is free from the doubts arising from his omitting any terms he ought to retain. This measure of the stress, of course, vanishes when the surfaces become parallel planes. But that does not prove that all stress vanishes; for if we calculate Maxwell's measure for the stress, we find that

$$\frac{P_2-p}{{}_1P}=\frac{c_1^2}{2c_2^2}\cdot\frac{\alpha_c-\alpha'}{\alpha'};$$

and when the surfaces now become parallel planes, this, so far from vanishing, attains its maximum value,

$$P-p=\frac{P}{2}\cdot\frac{\alpha_c-\alpha'}{\alpha'}.$$

I cannot believe that Prof. Reynolds overlooked the fact that the pressures on planes turned in different directions are unequal; but he does seem to have overlooked the fact that his measure of the stress is inadequate, and that the gas may be subject to stress, even though his measure for it vanishes; and he certainly cannot have been aware that his equations lead to the conclusion that there is a state of stress in the gas between two infinite parallel plane surfaces, one of which is hotter than the other, such that the pressure on each of them is different from that out sideways between them.

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In addition to this, Prof. Reynolds has not noticed the action of the tangential stresses in the case of the radiometer. That they play an important part in the phenomena is evident. For example, in the case of a small plane plate, hot on one side and cold on the other, represented in fig. 12, p. 836 of Prof. Reynolds's paper, if it were surrounded by a properly shaped and heated envelope, as far as Prof. Reynolds shows, the pressures all over the envelope might be the same; and so the envelope would have no tendency to move, owing to them, while the pressures on the plate would tend to move it. Such a state of affairs is manifestly impossible; so that it is evident that the tangential forces must be of importance. Maxwell, indeed, goes so far as to assert that if the whole stress were taken into account, and if there were no discontinuity near the surface of solids, then no motion would take place. This seems very likely, but is entirely opposed to the drift of this part of Prof. Reynolds's explanation. On p. 840 Prof. Reynolds italicizes the assertion, *The action of the radiometer does not depend on the distance between the hot and cold plates.* Yet, only four pages before, he asserts that these pressures do depend on the quantity of heat passing; and this even he will acknowledge depends on the distance between the hot and cold plates. He goes on to say that no action will take place, however close the plates are placed, if they be not of limited extent. If he means by no action no currents in the gas, he is certainly right; but if he means that there will be no inequalities of pressure, I think I have shown that this is contrary to the results of all his own equations, and was due to his overlooking the possibility of any stress besides the kind of one he happened to be considering.

Though Prof. Reynolds does not seem to agree with Maxwell that there would be no tendency to motion unless the discontinuity at the surface of solids produced currents of gas, yet his discussion of the motion of the radiometer seems almost to imply this. In conclusion, then, I think that Prof. Reynolds has done invaluable service to science by calling attention to the vital importance of taking account of the discontinuity at the surface of contact of the solid and the gas, and to the interesting phenomena that occur in consequence. Though I very much prefer Maxwell's method of calculation, as being capable of much greater extension and as savouring much less of assumption, yet Prof. Reynolds's has the great advantage that it is continually reminding one of the necessity for considering the discontinuity, which even Maxwell overlooked until reminded of its importance by Prof. Reynolds's able investigation.

40 Trinity College, Dublin,
January 8, 1881.

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XV. *On a Spectrum-Telescope.* *By* Dr. P. Glan*.

SINCE the discovery of methods for examining the solar prominences at all times, and since hitherto such examination has been confined to only small parts of the circumference at the same time, the efforts of physicists and astronomers have been directed to the simultaneous observation of larger portions of the sun's edge in monochromatic light. These efforts have become of more importance since the observation was made that the passage of a star behind the sun's limb takes place at different times, according as the sun is observed in the usual way (in white light) or in spectral homogeneous light. The latter method of experimenting, as is well known, consists in throwing an image of the sun from the objective of a telescope upon the slit of the spectroscope, and examining through the telescope of the latter a small part of the spectrum of that portion of the solar image which falls upon the slit. Even when the dispersion is great and the slit as wide as possible, we cannot by this means see more than a small part of the sun at one time. I shall describe here a method of observation which allows a considerably larger part of the sun to be seen at once in homogeneous light, and which permits the telescope to be used in the ordinary way, namely with its eyepiece and cross-wires—so that, for instance, it is easy to measure the sun's diameter as seen in the different spectrum-colours.

The method is the following:—Upon the eyepiece of a telescope I screw a small direct-vision spectroscope with collimator and telescope. In the latter, at the place where the spectrum is thrown, there is a movable diaphragm with a slit-shaped opening. The spectroscope is so adjusted that the Fraunhofer lines and the edges of the diaphragm are seen clearly at the same time. The eyepiece of the spectroscope is unscrewed, and the eye is placed immediately behind the slit of the diaphragm, after the slit and that of the collimator have been widened to about one third of a millimetre. The eyepiece of the telescope with the spectroscope screwed to it is then moved till the image of the sun becomes distinctly visible. The image of the sun thrown by the objective of the telescope thus gives rise, by means of the eyepiece of the telescope and the collimating lens of the spectroscope, to a second image, which in my apparatus falls immediately in front of the train of prisms. This image is seen enlarged through the objective lens of the telescope (which acts as a magnifying-glass), in

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that colour of the spectrum of the collimator's slit which, falling upon the diaphragm of the eyepiece, passes through its slit. I used a terrestrial telescope of Schmidt and Haensch of 50 millims. aperture; and by this means I was enabled to see nearly half the sun at once in homogeneous light. Seen by the naked eye, the sun appeared white in almost all parts of the spectrum excepting in the red, where it assumed a yellowish-white tint. This agrees with the observation that all colours become white if of sufficient intensity. The image at parts of the edge at right angles to the slit, to a distance of about 40° on both sides, was perfectly sharply defined. The other parts of the sun's edge were less sharp. These slight residues of lateral diffraction are probably chiefly due to the circumstance that every point of the sun's image gives a line-spectrum through the train of prisms, and that the slit-shaped opening before the eye allows a little line of this spectrum, and not a mere point, to pass through.

This residual diffraction, however, is only slight; and I hope to eliminate it further by future improvements in the apparatus. Further, it is possible to see every part of the sun's edge perfectly distinctly. For this it is only necessary so to turn the spectroscope that its slit stands at right angles to the edge under observation. Such an attitude of the slit would, for instance, be advantageous in observing the beginning of the transit of a planet. I have found that any diffraction which might be caused by the slits of the spectroscope is sufficiently removed by giving the slits a width of $\frac{1}{3}$ millim., or at most $\frac{1}{2}$ millim. If the slit be narrower than $\frac{1}{3}$ millim. the edges parallel to the slit appear very washed out; and when the slit is exceedingly narrow, the sun's image is pulled completely out broadwise. A slight diffraction may also have been caused by the use of a train of direct-vision prisms, which does not refract homocentrically the sun's image, which is in this case at a finite distance from it. I would therefore, in future, recommend the use of an equilateral prism, employing the angle of *minimum* refraction: this is always homocentric.

In the examinations of the sun which I undertook, from the 29th of January to the 10th of February, 1880, I was unable to detect any protuberant eminences on the sun's edge. This was no more than was to be expected, from the slight dispersion of the train of prisms. On the other hand, I have been able to observe satisfactorily the beginning and changes of sun-spots. I shall describe one observation. On the 4th of February I saw a large spot close to the upper edge, a second one of about the same diameter more towards the centre, and a group of three (two larger, and a smaller weak nebulous spot) to

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the right of them. Without the spectroscope, with white light I saw, in the case of the larger spots, the very black nuclear speck and the *penumbræ* with admirable sharpness. With the spectroscope there were also seen all the spots, and in some the nuclear specks and *penumbræ*, although they were not so sharply separated. In the methods hitherto described, when the spectroscope was added it was necessary to alter the adjustment of the telescope. If it were required to make use of the spectroscope and still leave the telescope totally unaltered in position, as it is usually adjusted with its micrometer cross-piece, an auxiliary lens has to be interposed immediately between the slit and the eye-diaphragm. With my telescope I used a lens of 67·5 millims. focal length. When I arranged the telescope in the ordinary way and then fastened to the eyepiece the convex lens and the spectroscope, one immediately behind the other, I was able to see the objects in homogeneous light as clearly as previously in white light, without having to alter the adjustment of the telescope in the least. Other lenses would have to be selected for other telescopes.

The arrangement of the apparatus hitherto adopted, which was made by Schmidt and Haensch in Berlin, was only a provisional one. The prisms and lenses of this spectroscope were selected without any special examination of their surfaces; and this may have contributed to deprive the homogeneous images of their highest possible sharpness. I hope to be able to have the apparatus made in a more complete form.

Addition (*Dec.* 21, 1880).

I add a few data concerning the focal length of the lenses and the dimensions of the spectroscopes employed. The focal length of the collimator-lens was 40 millims.; its free opening was 8 millims. The focal length of the objective lens of the telescope of the spectroscope was 44 millims.; its free opening was 12 millims. The train of Amici prisms was 75 millims. long. The total length of the spectroscope (without its eyepiece), which was screwed on to the eyepiece of the terrestrial telescope, was 165 millims., measured from the slit of the collimator to that of the eyepiece.

I may add the following to the previously described observations on the sun-spots. The spots, when viewed in orange, green, and especially in blue, homogeneous light, appeared distinctly darker than in red light. The difference appeared to me to be most noticeable when the air was exceptionally clear, and failed as the weather clouded. As this estimation depends upon judgments of the luminosity of the sequent

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images, such comparison is not quite trustworthy. On each of the four days, however, when I made observations in the different homogeneous colours, I believe that I noticed a greater darkness with the blue.

XVI. *On the Coefficient of Expansion of Gas-Solutions.* By E. L. Nichols, *Ph.D.* (*Göttingen*), *and* A. W. Wheeler, *Fellows of the Johns Hopkins University, Baltimore**.

[Plate II.]

In the determination of the coefficient of expansion of aqueous gas-solutions the methods used for ordinary liquids by Pierre, Kopp, Matthiessen, and others are open to serious objection. For the research about to be described here, which is confined to the study of the expansion of aqueous solutions of ammonia, the writers devised an apparatus better adapted to their purpose.

This apparatus consists of a bath, B (fig. 1), in which is immersed the glass bulb of the dilatometer, D. The straight portion, N, of the neck of the dilatometer dips at its lower end into a larger glass tube, T, filled with mercury. S is a mirror-scale graduated to millimetres. The neck N can be fastened securely to the scale at *k*. The mercury-tube is adjustable to different heights. The thermometer, *t*, shows the temperature of the bath. H is the stirrer.

With this apparatus there is no evaporation, nor escape of gas, the solution being kept entirely from the air. Aside from this advantage over the open-mouthed dilatometers is that of very great sensitiveness, due to the great size of the bulb compared with the capacity of the neck. The convenience in filling is also of importance when one is experimenting with volatile liquids.

Our method of volume-measurement was as follows:—The dilatometer was filled, then placed in the bath in the position shown in fig. 1 (the bulb being wholly below the surface of the liquid), and was securely fastened. The adjustable tube *t* was then raised until the end of the neck of the dilatometer dipped into the mercury. The filling was performed at a temperature higher than that of the bath, so that the mercury might rise somewhat in the neck. The bath was then cooled by intervals of a few degrees each; and the height of the mercury in the neck, after each cooling, was noted. From these readings and the corresponding temperatures our results were calculated. To obtain from these readings the relative volumes of the solution for different temperatures the following data were necessary:—

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(1) the volume of the dilatometer at some known temperature;

(2) the coefficient of expansion of the glass of the dilatometer;

(3) the relative volumes of the immersed and exposed portions of the dilatometer;

(4) the exact internal area of the neck at every point;

(5) the height of the mercury column in the neck;

(6) the temperature of the bath and of the surrounding atmosphere.

We made use of two dilatometers, identical in principle and differing but slightly in form, viz.:—

	cubic centim.
Dilatometer *a*, capacity at 15° C. . .	286·3425
„ *b*, „ „ . . .	282·6184
Coefficient of expansion of the glass .	·000024

The calibration of the necks by the method usually employed in calibrating thermometer-tubes was impracticable on account of the large diameter of the tubes. The method resorted to was as follows:—The dilatometer having been inverted and the neck at the bent portion stopped up, equal amounts of mercury, carefully weighed, were poured down the neck; and the height of the mercury column, after the addition of each such portion, was measured with the cathetometer. In this way a very satisfactory curve of cross-section areas was obtained and the capacity of each millimetre of the neck tabulated.

The solution within the dilatometer during each experiment was kept under constant pressure by means of the adjustable mercury-tube T, which was pushed up as fast as the mercury rose within the neck. The liberation of gas from the solution, which would inevitably have followed any considerable decrease of pressure, was by this means prevented.

The thermometers used were four in number:—

Baudin 7310, 7313, and 7315;

and

Golaz.

The variation of each of them from the air-thermometer was determined, and is given in our tables of observations. The Golaz thermometer showed an almost constant difference from the air-thermometer, while in the case of the Baudin thermometers this difference varied for different parts of the scale. In the bath, water was used for ordinary temperatures; and between 0° C. and −20° C. we employed the usual freezing-mixture of common salt and ice.

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To obtain with accuracy the temperature of a large bulb, by means of a thermometer in the bath outside, two precautions must be taken. The bath must be continually stirred, and after each change of temperature it must be kept at a constant temperature until the entire contents of the bulb shall have assumed that temperature. We found, by a series of preliminary trials, the time necessary to this end to be thirty minutes after a 1° to 2° change of temperature, and thirty-five minutes after a 5° change.

In our determinations five different solutions of ammonia gas were used. The specific gravity of each solution as obtained at the time of filling the dilatometer with the same, together with the strength of the solutions, appears in Table I.

TABLE I.

No. of solution.	Specific gravity at 14° C.	Percentage of NH_3 in solution.
1.	·9009	29·00 per cent.
2.	·9373	16·19 ,,
3.	·9676	7·96 ,,
4.	·9766	5·61 ,,
5.	·9913	2·12 ,,

Table II. gives the volumes of these different solutions for various temperatures, compared with the volume of each at 4° C. taken as unity, and calculated directly from our observations. The dilatometer *a* was used with solutions 1 and 2, and dilatometer *b* with solutions 3, 4, and 5.

TABLE II.—Volume Table. Observed Values.

Percentage of NH_3 in solution = 29·00 per cent.			Percentage of NH_3 in solution = 16·19 per cent.		
Temperatures.		Observed volumes compared with the volume at 4° as unity.	Temperatures.		Observed volumes compared with the volume at 4° as unity.
Baudin 7313.	Air-thermometer.		Golaz.	Air-thermometer.	
15°	15·00	1·007214	22°	21·80	1·007129
13	13·01	1·005835	17	16·80	1·004930
11	11·06	1·004547	12	11·80	1·002874
9	9·15	1·003303	7	6·80	1·001013
7	7·28	1·002064	2	1·80	0·999110
5	5·33	1·000862	− 3	− 3·20	0·997514
3	3·27	0·999618	− 8	− 8·20	0·996095
1	1·16	0·998350	−13	−13·20	0·994890
−1	−0·80	0·997109	−17	−17·20	0·994139
−3	−2·51	0·996020			
−5	−4·38	0·994838			
−7	−6·40	0·993585			

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Table II. (*continued*).

Percentage of NH_3 in solution = 7·96 per cent.			Percentage of NH_3 in solution = 5·61 per cent.		
Temperatures.		Observed volumes compared with volume at 4° as unity.	Temperatures.		Observed volumes compared with volume at 4° as unity.
Golaz thermometer.	Air-thermometer.		Mercury-thermometer.	Air-thermometer.	
15°·20	15°·00	1·002662	20°·00	19°·80	1·003180
10·00	9·80	1·001191	15·00	14·80	1·001953
5·00	4·80	1·000141	9·00	8·80	1·000726
0·00	− 0·20	0·999417	5·00	4·80	1·000106
− 5·00	− 5·20	0·998932	1·00	1·16	0·999594
−10·00	−10·20	0·998740	− 1·00	− 1·20	0·999396
−11·00	−11·20	0·998733	− 4·00	− 4·20	0·999296
−12·00	−12·20	0·998760	− 5·00	− 5·20	0·999272
−13·00	−13·20	0·998794	−10·00	−10·20	0·999289

Percentage NH_3 in solution = 2·12 per cent.		
Temperatures.		Observed volumes compared with volume at 4° as unity.
Mercury-thermometer.	Air-thermometer.	
15°	14°·75	1·001114
10	9·72	1·000413
5	4·76	1·000033
3	2·80	0·999958
2	1·80	0·999941
0	−0·20	0·999941
−5	−5·20	1·000296

From Table II. curves were plotted, with temperatures as abscissæ and volumes as ordinates (see fig. 2); and from these curves a table was compiled, giving volumes and coefficients of expansion for temperatures between +20° and −20° (see Table III.).

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TABLE III.—Volumes and Coefficients of Aqueous Solutions of Ammonia.

Temperatures.	Volumes.	Coefficients.	Temperatures.	Volumes.	Coefficients.
Percentage of NH_3 in solution = 29 per cent.					
20°	1·010505	·000682	0°	0·997875	·000636
18	1·009168	·000678	− 2	0·996442	·000629
16	1·007775	·000674	− 4	0·995190	·000621
14	1·006535	·000670	− 6	0·994029	·000614
12	1·005162	·000666	− 8	0·992925	·000606
10	1·003868	·000662	−10	0·991770	·000598
8	1·002595	·000658	−12	0·990730	·000589
6	1·001320	·000653	−14	.....	·000579
4	1·000000	·000648	−16	.....	·000567
2	0·998728	·000642	−18	.....	·000553
			−20	.....	·000538
Percentage of NH_3 in solution = 16·19 per cent.					
20	1·006355	·000440	0	0·998549	·000326
18	1·005445	·000429	− 2	0·997886	·000312
16	1·004628	·000418	− 4	0·997246	·000297
14	1·004230	·000407	− 6	0·996659	·000280
12	1·002885	·000396	− 8	0·996148	·000262
10	1·002172	·000384	−10	0·995639	·000243
8	1·001437	·000372	−12	0·995155	·000222
6	1·000730	·000361	−14	0·994718	·000200
4	1·000000	·000349	−16	0·994029	·000177
2	0·999267	·000338	−18	.....	·000153
			−20	.....	·000128
Percentage of NH_3 in solution = 7·96 per cent.					
20	1·004360	·000320	0	0·999405	·000135
18	1·003678	·000307	− 2	0·999174	·000118
16	1·002981	·000293	− 4	0·998978	·000086
14	1·002336	·000279	− 6	0·998865	·000060
12	1·001740	·000263	− 8	0·998788	·000033
10	1·001224	·000243	−10	0·998742	·000006
8	1·000778	·000223	−12	0·998748	−·000025
6	1·000368	·000200	−14	0·998925	
4	1·000000	·000178	−16		
2	0·999648	·000156	−18	Frozen.	
			−20		

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Table III. (*continued*).

Temperatures.	Percentage of NH_3 in solution = 5·61 per cent.		Percentage of NH_3 in solution = 2·12 per cent.		Water (pure)*. Percentage of NH_3 = 0·00.
	Volumes.	Coefficients.	Volumes.	Coefficients.	Volumes.
20	1·003228	·000264	1·002038	·000198	1·001744
18	1·002725	·000252	1·001653	·000187	1·001348
16	1·002205	·000236	1·001319	·000173	1·000999
14	1·001765	·000223	1·000919	·000155	1·000701
12	1·001338	·000206	1·000689	·000137	1·000451
10	1·000941	·000189	1·000432	·000116	1·000345
8	1·000585	·000172	1·000287	·000095	1·000114
6	1·000281	·000153	1·000120	·000071	1·000030
4	1·000000	·000132	1·000000	·000046	1·000000
2	0·999703	·000110	0·999945	·000017	1·000073
0	0·999500	·000087	0·999949	−·000017	
− 2	0·999362	·000064	1·000043	−·000058	Frozen.
− 4	0·999285	·000041	1·000173	−·000180	
− 6	0·999199	·000016			
− 8	0·999167	−·000013	Frozen.		
−10	0·999280				
	Frozen.				

Curves showing the coefficients of expansion are given in fig. 3. The temperatures are abscissæ, the coefficients ordinates. These curves are valuable in the determination of the points of maximum density of the solutions. They cut the base-line at those points, thus fixing the temperatures to within a small fraction of a degree; while from the volume-curves (which are parallel to the base-line at the points of maximum density) it is impossible to determine the latter with any accuracy.

The solutions of 2·12, 5·61, and 7·96 per cent. strength were frozen within the temperature-interval under observation; but the stronger solutions remained fluid to below −20° C. The following Table gives the saturation-points, points of maximum density, and freezing-points of the solutions in question.

* Rosetti's table, Poggendorff's *Annalen*, Bd. v.

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TABLE IV.

No.	Percentage of NH_3.	Saturation.	Maximum density.	Freezing.
1.....	29·00 per cent.	39·80 *	.	
2.....	16·19 ,,	59·00		
3.....	7·96 ,,	76·40	−10°·50	−14°·10
4.....	5·61 ,,	83·10	− 7·20	−10·60
5.....	2·12 ,,	93·20	+ 0·80	− 5·40 (?)
6.....	0·00 (water)	100·00	+ 4·00	0·00

We thus find that in the case of aqueous solutions of ammonia the freezing-point and point of maximum density are lowered by the presence of the gas in solution †. Before the law of this change can be given, many more experiments, covering a much greater range of temperature, will have to be made. The only attempts to freeze aqueous ammonia, of which we have found record, are those of Fourcroy and Vauquelin in 1797‡.

Their statement, which still finds place in our chemical text-books and dictionaries, is that a well-saturated solution (*bien saturée*) freezes at −40° C. Since the freezing-point is a rapidly changing function of the amount of gas in the solution, this statement is of little use without a knowledge of the strength of the solution in question. An examination of the curves in fig. 2 leads us to suspect that the solution frozen by Fourcroy and Vauquelin was the ordinary commercial solution of about 16-per-cent. strength.

Just as the determination of the freezing-point had, up to the time of the present work, been confined to observations upon a single solution, so with the study of the coefficients of expansion; we find but a single research on this subject (by Prof. Munke of Heidelberg, in 1828§). His observations, which extended to a variety of liquids, were made with the greatest care and exactitude; his study of aqueous ammonia, however, was confined to a single solution; so that no general conclusions concerning the influence of the strength of a solution upon its behaviour could be drawn.

* Roscoe and Dittmar, 'Quarterly Journal of the Chemical Society.'

† A similar lowering of the point of maximum density due to the presence of CO_2 gas in water has been noted by Mackenzie and Nichols in their paper "Ueber die Volumenvermehrung von Flüssigkeiten durch die Absorption von Gasen," Poggendorff's *Annalen*, n. F. iii. 1878.

‡ Fourcroy and Vauquelin, *Annales de Chimie et de Physique*, sér. 1, t. xxix.

§ Munke, St.-Petersburg Academy, *Mémoires par divers Savants*, 1831.

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The strength of Munke's solution was 13·75 per cent., having a specific gravity of ·9465 at 12°·5 C. The results of his measurements are shown in the curve marked "*Munke* 1828" (fig. 2). Although deduced from measurements taken by a different method, it falls into its proper place among the other curves in the figure.

If we take the volumes given in Table II. or Table III. for any one temperature and plot a curve, using these volumes as abscissæ and the corresponding percentages of gas contained in the solution as ordinates, we obtain a *straight line*—a fact which finds its interpretation in the following law, viz.:—

When ammonia gas is absorbed in water, the increase in volume of the solution for a constant temperature is directly proportional to the amount of gas absorbed.

Since for CO_2 gas the same law has been found true*, we feel warranted in suspecting it to be a general one.

The series of curves in fig. 4 show the variation in volume of various ammonia-solutions between −20° C. and 100° C. Given the saturation-point of the solution, the amount of gas which water is capable of absorbing at all temperatures, and the law just stated, the curves can be easily calculated. From the point of saturation to 100° C. the loss of gas more than counterbalances the expansion due to heat, and the volumes decrease rapidly.

Stated briefly, the effect of NH_3 gas in solution upon the water absorbing it is as follows:—

(1) *The coefficient of expansion of the solution is increased by the presence of the gas.*

(2) *The increase of volume due to the absorption of a gas is directly proportional to the amount of gas absorbed.*

(3) The point of maximum density of water is lowered by the presence of NH_3 or CO_2 gas in solution.

(4) The freezing-point of water is lowered by the presence of the above-mentioned gases.

These results seem to us to be opposed to the opinion commonly held by chemists, that NH_3 and CO_2, when in aqueous solution, form chemical compounds of the form NH_4HO and H_2CO_3. The effect of these gases upon the properties of the water absorbing them shows so complete an analogy to the changes occurring when salts are dissolved, that we are inclined to consider the products of gas-absorption to be true solutions rather than chemical combinations.

* Mackenzie and Nichols, article already cited.

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XVII. *On Alternating Discharges* in vacuo. *By* Dr. K. Domalip*.

Gassiot† was the first to observe and describe reciprocating luminous electric currents. He observed that, upon connecting the poles of a Ruhmkorff's coil with tinfoil coatings on the outside of a vacuum-tube, instead of passing the current directly through it, he obtained a finely stratified luminous discharge between the two coatings, which under the influence of a powerful magnet resolved itself into two similar portions, in which the stratifications were curved with the concavities in opposite directions.

Similar double currents are also obtained by simply connecting one of the two coatings with one of the two wires of the Ruhmkorff's coil. The current becomes stronger when the second coating is connected with the earth.

These double currents, as is well known, are easily explained. When the current is excited, the positive electricity becomes accumulated on the one coating, and the negative on the other. The positive electricity acts inductively on the electricity of the tube, fixing the negative electricity and setting the positive free. The opposite action takes place on the other tinfoil coating, the positive electricity becoming fixed and the negative free. The electricities set free in the tube at the two coatings unite, and form the first current. The opposite electricities accumulated on the exterior of the tube next unite through the coil, and the electricities condensed on the interior surfaces of the tube are set free and unite, forming the second current, which is opposed in direction to the first. The two currents are consequently not simultaneous, but consecutive.

Plücker‡ also obtained reciprocating currents, by using a long and highly exhausted tube into one end of which an electrode was fused. When this electrode was connected with one pole of a Ruhmkorff's coil, whilst the other end remained free or was placed in communication with the earth, currents were obtained which were double, as could be shown by means of an electromagnet, under the action of which the course of the one current can be distinguished from that of the other; the two currents are of equal intensity.

Plücker obtained also alternating currents, by connecting with the earth any point of a tube through which the direct current from the coil was passing; even the inductive action of the hand was sufficient to produce such double currents.

* Translated from a separate impression communicated by the Author.
† Proc. Roy. Soc. March 4, 1858. ‡ Pogg. *Ann.* vol. cvii. p. 77.

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These induced currents may be conveniently observed by employing a tube in which the "dark space" is much extended. If, then, the glass wall at any point of this dark space be placed in connexion with the earth, the luminous currents which are formed between the point touched and the nearest electrode can be easily observed, appearing to issue from the electrode and approach the point touched in the form of an arc. But when we employ a tube in which the vacuum has reached such a point that fluorescent light appears at the negative pole, the negative light of the induced current fills the whole section of the tube and strikes the opposite wall, when it sometimes produces a fluorescent surface.

These two induced currents differ from each other in the same way as the direct currents in the two well-known Crookes's bulbs, which serve to show the different distribution of the current in a space in which the air is moderately exhausted, and in one in which it is very highly exhausted.

Reitlinger* first observed certain phenomena with such tubes—first with a bromine-tube, and then with other tubes. When the finger, or any other conductor, is brought near the part of such a tube in which the brush-discharge is visible, there is seen on the glass wall opposite the finger or conductor a sharply-defined luminous green surface. If the finger is brought nearer, this bright green light assumes the form of a line thick in the middle and pointed at the top and bottom. Reitlinger believes that this is not a case of fluorescence, but of some other peculiar production of light in the glass of the Geissler's tube. He supposes this to be confirmed by the fact that often, when the light in the tube is stratified, it shows no change, but follows its course along the centre of the tube undisturbed by the approach of the finger, causing the green light on the opposite wall. The magnet may even be used to drive the luminous column of gas up against the finger, and nevertheless the green light on the glass of the opposite wall shows no change; so that it cannot be produced by any fluorescent action of the luminous column of gas.

Goldstein† has further examined this phenomenon, and observed that it may be completely explained by assuming that the inner surface of the portion of the glass wall touched by the conductor behaves exactly like a kathode, and consequently emits light possessing all the properties known to characterize the kathode-light.

The fluorescent phenomena observed by Reitlinger and Urbanitzki are to be regarded as excited by the kathode-rays

* *W. Sitzber.* vol. lxxiii. p. 691.
† *W. Sitzber.* vol. lxxiv. p. 463.

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issuing from the point touched, so soon as the exhaustion is sufficient for them to reach the opposite wall of the tube.

In what follows, I propose to describe experiments with the induced currents obtained in a high vacuum when the glass wall is touched with a conductor at any point.

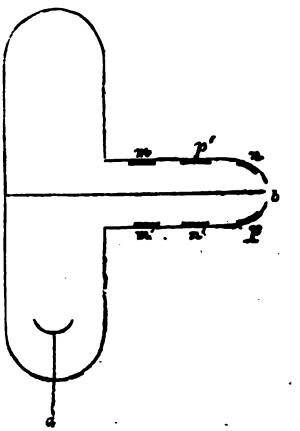
I observe, first of all, that these induced currents may be obtained in any tube which has been exhausted so far that the glass wall in the neighbourhood of the kathode fluoresces under the influence of the negative light, and that these currents may be obtained by touching the tube with a conductor at any point whatever. A Geissler's tube marked SiCl showed these currents not only in the neighbourhood of the kathode-space, but also in the positive space, whilst on touching any point of the glass wall a fluorescent surface appeared on the opposite side of the tube. It is advantageous to include an air-spark in the circuit; the longer this spark is, the more distinctly do the induced currents appear.

In all the tubes in which the vacuum is more nearly complete (for example, in Crookes's bulbs) these induced currents are much more intense.

Rectilinear Propagation of the Induced Currents.

For these experiments I used, first of all, a vacuum-tube such as was employed by Crookes to show that an opaque body placed in the path of the rays issuing from the negative electrode throws a shadow on the posterior glass wall. The tube, constructed by Götze, differed from that employed by Crookes by having the cross suspended upon an aluminium wire which crossed the tube, and upon which it could be made to slide into a lateral enlargement of the tube (fig. 1). When sparks from the coil are passed through this tube, *a* being connected with the negative pole and *b* with the positive pole, and a spark-length of 6 to 8 centims. being included in the circuit, brilliant fluorescence was observed on the part of the tube opposite the point touched, while at the same time the light causing this fluorescence could be seen in the tube itself, the rays spreading out from the inner surface of the tube at the point touched to the opposite surface.

Fig. 1.



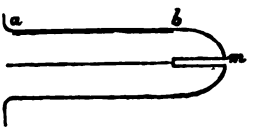
If, for example, the tube were

touched at the point m, the rays issued at right angles to the surface and impinged on the opposite wall at m'. If, on the other hand, the tube were touched at n or p, the rays impinged on the inner surface at n' or p', at which points bright fluorescence appeared. In all cases it appears that the rays issue at right angles to the elements of the surface touched, and radiate into the vacuum-tube rectilinearly till they strike upon the glass wall, where the well-known fluorescence appears. It is easy to further convince oneself of this normal and rectilinear propagation by touching the tube at other points, or by using for this purpose other vacuum-tubes of different form, but in which the vacuum is sufficiently good. Hence it may be concluded from these experiments that these induced currents radiate in the same way as the direct current of the coil in the same vacuum.

Induced Currents throw Shadows of Bodies in their Path upon the opposite Wall.

For these experiments the tube with the lateral enlargement, already described, is well suited, through the middle of which an aluminium wire passes, terminating at m in a narrow glass tube. If the entire distance $a\,b$ is placed in connexion with the earth, there is formed on the opposite wall a shadow of the wire, which is surrounded by vivid fluorescence. This formation of shadows may be observed with most of Crookes's radiometers. I have employed for this purpose an ordinary radiometer with a mica fly. The bulb, as is well known, is drawn out in the direction of a vertical diameter into a tube of suitable length, which is then placed in a little wooden stand. This tube was surrounded on the outside with a strip of tinfoil near the bottom, which serves as the one electrode; a second disk-shaped electrode of tinfoil was fastened on the outside of the glass bulb at the point where a horizontal diameter is intersected by the glass wall. The two electrodes were either connected with the poles of a Ruhmkorff's coil, or one was connected with the rheomotor and the other was put to earth. In both of these cases the rays which traverse the bulb in the direction of a horizontal diameter fall upon the fly of the radiometer, and a corresponding shadow of the fly is seen upon the opposite wall of the bulb surrounded by green fluorescence. E. Wiedemann* has observed a similar phenomenon in the use of a spherical positive electrode contained in a spherical glass bulb: when a point of the exterior wall of

Fig. 2.



* *Ann. d. Physik u. Chemie*, n. F. vol. ix. p. 160.

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the bulb is touched with a conductor, a distinct shadow of the electrode surrounded by brilliant green fluorescence is formed on the opposite wall of the bulb.

Mechanical Action of Induced Currents.

Crookes, as is well known, has discovered a new mechanical action of the rays issuing from the negative electrode, and has succeeded in setting in motion a little wheel with vanes upon an inclined plane by their action. The question presented itself whether these induced currents would be sufficiently powerful to produce a similar mechanical action. For this purpose a vacuum-tube made by Götze was employed. This tube, whose length was 18 centims. and breadth 4 centims., was furnished with a radiometer in the middle, and had at one end an aluminium electrode so placed that the vanes of the radiometer would be struck by the rays issuing from this electrode when made the negative pole, and would thus be set in motion. The second electrode at the other end of the tube is surrounded with a glass tube extending 2 or 3 centims. beyond the aluminium electrode, and then bent at a right angle towards the side of the vacuum-tube. When this electrode is connected with the negative pole of the induction-coil, the rays issuing from it can no longer impinge upon the vanes of the radiometer, which therefore is not set in motion. But in this case the radiometer is easily put into motion in either direction by touching one point or another of the glass wall with a conductor. The rotation is easily explained by the production of induced currents on connecting the exterior surface of the glass with the earth. Currents are produced which radiate from the surface corresponding to the point touched in straight lines into the vacuum-tube, and thus strike the vanes of the radiometer and put it into motion in a definite direction. If then the tube be touched at another point so that the currents thus produced strike the other half of the radiometer, we have motion in the opposite direction produced.

This mechanical action may also be shown with an ordinary Crookes's radiometer coated at two points of the exterior surface with tin-foil, as already described, when the coatings are connected with an induction-coil.

Action of the Magnet.

In order to investigate the action of the magnet on these currents, the vacuum-tube described above with the aluminium cross was employed. Whilst the primary induction-current passed through the tube, the induced currents were excited

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by connecting the upper surface of the side tube with the earth. Then, as we know, these currents are propagated in straight lines from the surface touched and strike the opposite wall, producing a fluorescent surface. The production of this fluorescent surface much facilitates the investigation into the action of the magnet, since it is only necessary to observe the motion of the fluorescent surface in order to infer the motion of the current producing it. In some cases the rays issuing from the points touched are visible; but in other cases they are not.

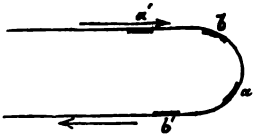
The action of the magnet in the equatorial position was first investigated. For this purpose the tube was so placed between the poles of the magnet that the line joining the point touched with the fluorescent surface on the opposite wall was at right angles to, and bisected, the line joining the two poles. When the magnet was excited, the fluorescent surface, and with it the current producing it, would move upwards or downwards according to the polarity of the magnet. Closer examination showed that the deviation could be determined by assuming that the negative current moved in a straight line from the point touched to the opposite wall, and that consequently the positive current is propagated in the opposite direction. Hence, applying Ampère's rule, we can easily explain the motions observed.

Next, the magnetic action was investigated when the current was placed axially: the tube was moved along in the axial position, so that the current was first over the one pole; then, on moving the tube further, it came into the neutral point, and then over the other pole. In every case the motion of the fluorescent surface, and consequently of the current, could be predicted from Ampère's rule.

It is not, therefore, necessary to describe the different cases; but I will proceed to describe another experiment.

When the side tube of the vacuum-tube, already described, is touched at two different points, a and b, two intersecting currents will be produced; b' is the fluorescent surface caused by the current proceeding from b, and a' that produced by the current from a. When this tube is placed equatorially between the poles of the magnet, the fluorescent surfaces are displaced in opposite directions on the glass wall, in accordance with Ampère's well-known rule.

Fig. 8.



I will further remark that, in order to observe these motions

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under the influence of the magnet easily, the tube should be slowly brought near to the magnet from a distance. From what has been said, it follows that these currents possess the ordinary properties of currents, since their deviation is the same as of any conductor whatever conveying a current in the same direction. The action of the magnet upon these induced currents agrees completely with the action of the magnet upon the direct current in a highly exhausted space *, as I have previously found.

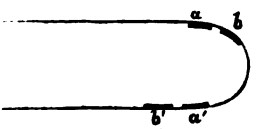
Electrodynamic Actions.

Goldstein, in the research already quoted, describes certain phenomena which are obtained when the tube is connected at two or more points with the earth. In this case the surfaces of light are not necessarily opposite the points touched, the position of each light-surface is dependent on the magnitude and relative position of the surfaces touched.

These and all other similar phenomena are easily explained by the mutual electrodynamic action of the currents produced by connecting the different points of the glass surface with the earth. We have in fact to deal with the same action as would be exerted upon each other by currents traversing solid movable conductors. As with these, so also with the induced currents, we observe that similarly directed currents attract each other, and oppositely directed currents repel each other. These experiments were varied in different ways; but all the phenomena are easily explained by the electrodynamic mutual action of the currents produced. I will therefore, in what follows, only describe one or two experiments which will justify what has been said.

The vacuum-tube with the aluminium cross was again employed. When a powerful current was passed through the tube from the coil, the aluminium wire m being the positive pole, then when a was connected with the ground, a fluorescent surface appeared on the opposite wall at a'; when b alone was touched, the fluorescent surface appeared at b'; but when both currents were excited simultaneously, the fluorescent surfaces approached each other. The currents in this case therefore attract each other. The mutual attraction can be easily observed by keeping a connected with the earth, and alternately making and breaking contact with b;

Fig. 4.



* *Sitzb. d. k. Akad.*, vol. lxxxi. p. 640.

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the approach of the fluorescent surface a' to b' when contact is made, and its return to its original position so soon as contact is broken, can be easily observed.

This attraction is very easily explained. When the points a and b are touched, currents are excited, as we know, which start from the corresponding points of the inner wall of the tube, and propagate themselves in straight lines through the tube in the directions a' to a and b' to b (if we take account only of the positive electricity); we obtain, therefore, in this case two similarly directed and intersecting currents, which, as we know, attract each other and bring about the approach of the fluorescent surfaces.

If we again touch the tube at a so that the fluorescent surface appears at a', and if then the tube be also touched with a conductor at a point close to a', we observe a repulsion of the fluorescent surface. In this case also the motion is most easily observed by maintaining contact with a, and alternately making and breaking the other contact. In this case we have the mutual action of oppositely directed currents which, we know, repel each other. When contact is made with more than two points of the tube, the relative position of the surfaces is altered. This alteration can, however, easily be explained by well-known electrodynamic action.

I take this opportunity of pointing out the want of agreement in the result of this experiment with that of Crookes's experiment. Crookes passed two similarly directed currents through a highly exhausted tube, having at the one end two electrodes side by side, both connected with the negative pole, and at the other end only one electrode, which was made the positive pole. We obtain then two similar currents traversing the vacuum. By the use of a properly adjusted screen with two openings near the negative electrode, Crookes obtained two well-marked currents, the paths of which were observed by means of a fluorescent screen. Both currents were found to deviate from their normal path in consequence of repulsion. It would be of great interest to repeat this experiment, and to investigate, by means of such a tube, the different action on each other of the induced currents, and especially their electrodynamic action; but, unfortunately, I have not yet been able to obtain such a tube.

The properties and action of induced currents investigated in this paper afford an explanation of the phenomenon first observed by Reitlinger with certain tubes, viz. the so-called repulsion of the direct current of the tube when the tube is touched with a conductor, since the repulsion thus observed is seen to be due to the mutual action of the direct current and

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the induced current produced by the contact of the conductor.

The experiments described have been carried out in the Physical Museum of the University of Prague.

XVIII. *On Action at a Distance.* *By* WALTER R. BROWNE, *M.A., M. Inst. C.E., late Fellow of Trinity College, Cambridge**.

THE three several communications in the January Number of the Philosophical Magazine, commenting on my paper upon "Action at a Distance," seem to demand some reply on my part. But I may be permitted to observe that the gist of that paper lay in a few short pieces of purely physical reasoning, designed to prove that certain known facts not only were not, but could not be explained on the hypothesis that action at a distance was impossible. Now not one of my three opponents has pointed out a flaw in these reasonings. In fact, they all appear to imply that in some of them at least they can find none; therefore I do not think I need take up much time in answering any counter arguments they may employ.

I will, however, devote a few words to each. Taking them in reverse order for convenience, I may point out that Mr. Allen's quotation from Clerk Maxwell has nothing to do with action at a distance as compared with action by contact, but only with action at great distances as compared with action at small distances, Clerk Maxwell himself taking care to observe that the one is really just as mysterious as the other. Mr. Allen's own idea of an impalpable fluid will not serve, until he shows how the laws of fluids are accounted for by impact alone. It seems needful to insist on the fact that it is not the action of molecules which is under discussion, much less that of ordinary bodies, but the action of *atoms*, using that word in its strict sense to designate those ultimate and individual elements of matter, which no known power can divide, destroy, or alter in any way. This conception must not be confounded, for instance, with that of the vortex atom, which, it is true, forms a whole that is not broken up by any natural process, but which by definition contains an immense number of ultimate atoms, each having a separate motion of its own.

Mr. Tolver Preston should explain what he means by explanation. I know no meaning for that word except that assigned to it by Mr. J. S. Mill and others, viz. the

* Communicated by the Author.

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reduction of complex facts under simple principles, of particular cases under general laws. The ultimate and most general principles must therefore be for ever unexplained. Far from its being true that "that which is in itself inexplicable cannot explain any thing," nothing is fully explained until it has been brought under an inexplicable law. Mr. Tolver Preston fails to recognize that the action of bodies in contact, taken as an ultimate principle, is precisely as inexplicable, as "occult and mystical," as the action of bodies at a distance, and that this is perfectly clear to many competent judges. I will cite only one, whose competency no one can deny. Prof. Cayley, in a recent letter to me, remarks:—"My own view has always been that the action of two bodies at a distance presents no greater difficulty than that of two bodies in contact."

Mr. Tolver Preston suggests that the compression of a bar of iron may be explained by the fact that its molecules are elastic, though he does not suggest any explanation of elasticity under the theory of contact. I have already pointed out, as I did in my first paper, that the question is not about molecules, but about ultimate atoms; and therefore there is no point in the above remark. Beyond this, Mr. Tolver Preston makes no objection to my reasoning, and in the case of magnetism he appears to admit its truth; and yet he demands that the impossibility of action at a distance shall still be admitted. To claim assent to a theory because a certain number of facts may be explained in accordance with it, whilst admitting that there are other facts with which it is at variance, is a proceeding which I had hoped was unknown in the domain of pure physics. In that domain progress has only been attained by rigorously rejecting every hypothesis the moment it was shown to be inconsistent with a proved fact; and on this ground I claim the rejection of this particular hypothesis of universal contact action.

On the side issue as to Le Sage's theory of gravitation, I need only say a word. My remark as to the porosity of matter was independent of the fact whether the spaces left were within the molecules or between them. I was aware that Mr. Tolver Preston had suggested the former; but it makes no difference in the result. With regard to the speed of the gravity-gas particles, I guarded myself by using the words "practically" and "indefinite" from the assertion that it was really infinite. It is sufficient for my purpose that we have no right to put any known superior limit to it whatever.

I come lastly to Dr. Lodge, whose note, though imposing

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in appearance, contains so many errors that I can hardly believe I have read it aright. In the first place, he considers my paper as valuable in pointing out metaphysical difficulties, whereas my only allusion to metaphysics was in repudiating the metaphysical notion that action at a distance is *à priori* inconceivable.

Secondly, he is fatally wrong in his statement of the principle of the conservation of energy. The work done on a body B is *not* "measured by the product of the force exerted into the distance moved through in the direction of the force;" on the contrary, it is measured by the product of the resistance into the distance moved through against that resistance. Suppose we have a weight A of 1 ton acting on a smaller weight B over a pulley. Will Dr. Lodge assert that the work done by A, when it has fallen through 1 foot, is precisely the same whether the weight of B is 1 cwt. or 19 cwt.?

Thirdly, Dr. Lodge talks of equal and opposite forces, and of equal and opposite works. What he means by these terms, except that the motion due to such forces or works is in opposite directions, I am unable to divine; and yet that is the point he is trying to disprove. That there can be no transfer of energy due to motion of two bodies in the same direction, is shown in a moment. First, let the velocities of the two bodies be equal, and impress upon them a velocity equal and opposite to this common velocity. This, by the second law of motion, will in no way affect the case. But the two bodies are now reduced to rest; and Dr. Lodge will not assert, I presume, that any transfer of energy is taking place between them. Secondly, let the velocities be different, and let them be $V+v$ and $V-v$. Impress on both a velocity $-V$; then the bodies are left with velocities $+v$ and $-v$; that is, with velocities tending in opposite directions.

Fourthly, Dr. Lodge is obviously in error in assuming that when the earth pulls a stone, any work whatever is done upon either. This is at once apparent from the fact that there is no resistance to the motion (neglecting, of course, the friction &c. of the air). All that happens is, that in each case a certain quantity of potential energy is turned into actual energy. The total energy of each remains the same, and no transference whatever takes place between them.

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XIX. *On the Friction in Free Surfaces of Liquids.* By A. Oberbeck*.

1. In his fine investigations on liquids which are withdrawn from the influence of gravity, Plateau† discusses the question why only a few liquids are suitable for the production of thin films, while most of them are totally unsuited for that purpose. According to his view, two properties of liquids play an essential part therein—the tension and the viscosity of their surfaces. The investigation of this second property is the aim of the present memoir.

As is well known, what is designated as the viscosity of a liquid is its deviation from the state of perfect fluidity, and manifests itself by a frictional resistance exerted by different quickly moving neighbouring parts of the liquid upon one another. From experiments which are subsequently to be more fully communicated, Plateau inferred that the amount of this frictional resistance varies according as the motion of the parts takes place in the interior or in the nearest vicinity of the free surface of the liquid; so that we have to distinguish an internal and a superficial viscosity. As the hydrodynamic differential equations with respect to the friction are at present in excellent accordance with all the known facts, and in them the friction has for its expression a single characteristic constant for each liquid (the friction-coefficient), the phenomenon discovered by Plateau might also be expressed in the following form:—The friction-coefficient is indeed a constant in the interior of a liquid; but at very small distances from the free surface it is a function of the distance from the free surface.

All the experiments which have been made upon free liquid surfaces teach that the liquid parts in them are in essentially different conditions from those in the interior; hence it would be quite conceivable that their mutual friction also is different. But it has been held necessary (by Poisson) to assume that the density rapidly changes very near the free surface. Although hitherto neither an experimental proof of this has been produced, nor even theoretically is there any occasion to designate this assumption as a necessary one, yet a variation of the friction-coefficient would include it as a self-evident consequence.

The friction of two different liquids against each other is

* Translated from Wiedemann's *Annalen*, 1880, No. 12, pp. 634–652.

† Plateau, *Mém. de l'Acad. de Belgique*, 1868, xxxvii. pp. 1–102; Pogg. *Ann.* cxli. pp. 44–58 (1870).

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essentially different from the friction in the interior of a liquid*. Here also a similar view might be taken—namely, that the friction-coefficient in the interior of the first liquid has a constant value, rapidly changes on approaching nearer to the boundary surface, and on the other side again assumes that value which belongs to it for the second liquid.

These considerations show that the superficial viscosity is absolutely incompatible with the notions we have hitherto entertained of the nature of liquids. Whether it actually exists, and therefore whether it is justifiable to assume a different value of the coefficient of friction at the surface from that in the interior, can of course only be decided by experiment.

2. Plateau's fundamental experiments on surface-viscosity consisted in observing the time taken by a magnetic needle to fall back from a deflection of 90° to the magnetic meridian. For this different times were necessary, according as the motion of the needle took place in the free surface or in the interior of the liquid. If the time in the first case was greater than in the second, Plateau inferred a greater—if the contrary, a less viscosity in the surface. Thus, according to Plateau, water, aqueous salt-solutions, glycerine, and especially solutions of albumen and saponine in water are liquids in which the friction at the free surface is greater than in the interior. Conversely, with alcohol, oil of turpentine, ether, bisulphide of carbon the internal viscosity is greater than the external. Finally, by mixing water and alcohol a liquid could be produced in which the two observed times are equal. Plateau has also endeavoured in a somewhat different manner to ascertain numerical values for the difference between the friction in the interior and at the surface. He observed the angle to which the needle, in falling back to the meridian, moved beyond its original position of equilibrium. From this, on the assumption that the resistance of the liquid is proportional to the square of the velocity, numerical values for the differences of viscosity in different cases can be calculated. Plateau himself, however, admits† that they can make no claim to precision.

After the publication of these experiments the surface-viscosity was investigated by Luvini‡ and Marangoni§; and the result arrived at by the latter was, that a distinction between internal and superficial viscosity is not justified. He

* *Cf.* O. E. Meyer, Pogg. *Ann.* cxiii. pp. 68, 441 (1861).
† *Mém. de l'Acad. de Belg.* xxxviii. p. 76 (1868).
‡ Phil. Mag. [4] xl. pp. 190–197 (1870).
§ *Nuovo Cimento*, [2] v.–vi. pp. 239–273 (1872), [3] iii. pp. 50–68, 97–115, 192–212 (1879).

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believes rather that the processes observed by Plateau, especially the retardation of the motion of the magnetic needle at the surface of some liquids, were occasioned partly by capillary actions in consequence of the alteration of the form of the free surface, partly by its being contaminated with foreign substances. In particular, he assumes that the latter form a thin elastic layer on the surface, which, on the motion of a body in the surface, oppose a species of elastic action. In his replies*, Plateau maintains his original view. Instead of entering into a criticism of the conflicting opinions, it seemed to me more important to examine the phenomena in question afresh by a different method. This method had to fulfil the following conditions:—

(*a*) The effects of friction shall admit of being separated from capillary actions at the surface, or from any surface-elasticity which (according to Marangoni) may be present;

(*b*) The frictional resistances shall be expressed by numerical values which can really be regarded as a measure for them;

(*c*) These numerical values shall be comparable, both in the interior and at the surface of the same liquid, and also in different liquids under like circumstances.

It is easy to understand that the usual methods for the determination of the coefficients of friction could not be employed here. In the flow of liquids through capillary tubes the free surface does not at all come into consideration. Moreover even the rotational oscillation of a disk is by no means adapted for the investigation of the phenomena with which we have here to do. If it be caused to oscillate at different distances from the free surface, O. E. Meyer† has observed that the frictional resistances diminish on approaching it; and this is exactly what was to be expected according to the theory of these oscillations unfolded by O. E. Meyer‡. According to it, by far the larger portion of the action upon the disk depends on those liquid particles which are vertically over or under the disk, and not on those which lie in the same horizontal plane with it. If the layer of liquid above the disk is very thin, it follows the oscillations of the disk irrespective of the value of the coefficient of friction.

Lastly, Plateau's method is well suited to demonstrate the phenomenon qualitatively; but it does not permit us to decide whether the longer time taken by the magnet on the surface for its return to the position of equilibrium proceeds from augmented friction or from other counteracting forces.

* *Bull. de l'Acad. de Belg.* [2] xxxiv. pp. 401–419; xlviii. pp. 106–128 (1880).

† Pogg. *Ann.* cxiii. p. 415 (1861). ‡ *Loc. cit.* p. 62–67.

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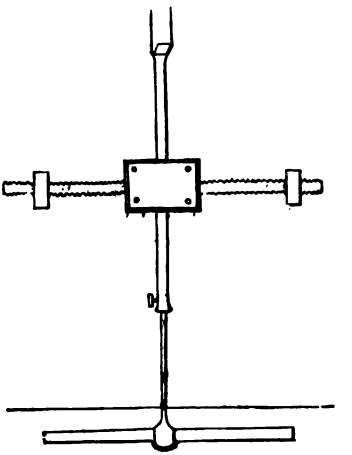
3. Hence, after a long series of preliminary experiments, I employed the following method, which, on the whole, satisfies the requirements above stated. Suspended by two fine, well-annealed platinum wires, a brass cross hangs bifilar, carrying a small mirror for the observation of the oscillations with scale and telescope. The two horizontal arms of the cross are furnished with screw-threads, on which suitable weights can be shifted, so that the oscillation-period can be varied at pleasure.

To the descending part suitable bodies can be fastened, which are destined to execute oscillations within the liquid (see fig. 1). I have used for this purpose chiefly thin plates or cylinders of brass. The entire apparatus is suspended on a suitable frame, and with the aid of a micrometer-screw could be raised and lowered at pleasure without being at the same time rotated about its vertical axis. On the micrometer-screw a rise amounting to only 0·01 millim. could be read off. Lastly, a small magnetic needle was attached to the apparatus, with the aid of which the above-described mechanism could be set in steady rotatory oscillation. The oscillation-period and the diminution of the oscillations can then, in the well-known manner, be determined with greater precision. The experiments were executed as follows:—The plate was at first entirely immersed in the liquid, and its upper margin was at a determined distance from the free surface; it was afterwards raised till its upper margin was in the free surface or projected a determined distance above it.

Fig. 1.



In both cases the liquid opposed a certain resistance to the plate, which produced a diminution of the amplitudes. If the friction of the parts of the liquid in the free surface is greater than in the interior, this resistance will also be greater. But in order to observe it in the most favourable form, it was necessary to give to the vessel destined to hold the liquid the most suitable shape possible. It was rectangular, 150 millims. in length, and 30 millims. wide. The long sides consisted of glass plates. Besides, it was so contrived that two other glass

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plates could be inserted in the vessel, parallel to the first, so as to permit observations to be made with the side plates at different distances. This arrangement was found necessary for the following reasons. If the brass strip turns round a vertical axis in the interior of a laterally unlimited liquid, it first and immediately puts in oscillatory motion that quantity of liquid which fills a cylinder whose diameter and height are determined by the length and height of the strip. It is self-evident that, in consequence of friction, the adjacent liquid also partakes of the motion, although at all events with rapidly diminishing strength. If the upper edge of the strip lies in the free surface of the liquid, a moving disk, of the dimensions indicated, will to a certain extent be separated, which will be chiefly although not exclusively set in motion. Friction in the surface will take place principally at the margin of this disk, where moving and nearly still layers border on one another. Hence it was to be feared that herewith the surface-viscosity that might be present would exercise but little influence. It is different when the liquid is inclosed by two not widely distant plates, to which it adheres. The action of friction, and especially of augmented friction at the surface, must then become evident. Plateau himself, in his experiments cited above, intensified in a similar way the action of the friction. I will subsequently communicate a series of experiments in proof of the correctness of the whole of these speculations.

4. The motion of the liquid under the influence of the oscillating plate is in any case tolerably complicated; and for the moment it seems to me impossible to calculate it from the general hydrodynamic equations. Even its reaction upon the oscillating system probably takes place not according to a law which could be simply rendered by a mathematical expression. I am therefore far from regarding the diminution of the oscillations at once as a measure of the friction-coefficient. But if two experiments be made, differing only in this, that in one of them the immersed plates cut the free surface with their upper margins, while in the other they are sunk a fraction of a millimetre deeper, then the resistances to the motion cannot be different unless the surface-layers exert a special influence.

As in all the experiments I confined myself to oscillations of small amplitude, it might readily be supposed that the liquid would chiefly produce a resistance proportional to the angular velocity of the apparatus, so that also for the oscillation of the system when the plate was immersed the simple equation

$$\frac{d^2\phi}{dt^2} + 2a\frac{d\phi}{dt} + b.\phi = 0 \quad . \quad . \quad . \quad . \quad (\text{I.})$$

would hold good.

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This assumption can easily be tested. It follows therefrom that oscillation-period and decrement are independent of the amount of the amplitude. This is not precisely correct; only the oscillation-period is constant, while the decrements diminish slowly with the magnitude of the amplitudes. I have not pursued the more precise law of this decrease further; the knowledge of the mean value of the decrements suffices perfectly for the decision of the question put; and I have taken care to arrange the experiments so that the decrements correspond to average amplitudes.

If the margin of the plate projects above the free surface, another disturbing circumstance comes in: the surface between the plate and the glass side is perceptibly curved, and in general raised somewhat above the level. During the oscillations this elevation is no longer the same on both sides; consequently attractive, and sometimes also repellent forces appear, which lengthen or shorten the oscillation-period. Here, however, with small amplitudes the oscillation-period is independent of their magnitude; so that the supervening forces must be regarded as proportional to the angle ϕ. The alteration of the oscillation-period is an excellent means of measuring accurately the forces mentioned. I have instituted a series of experiments in this direction, which I intend to communicate in a separate paper. Lastly, if in the surface a thin layer which (according to Marangoni's assumption) is elastic were present, a displacement or bending of it must at all events produce also a resistance, which, in a first approximation, would be proportional to the angle of deviation.

It follows from these considerations that, when the plate is removed from the interior of the liquid to the surface, in equation (I.) both a and b change. But as the oscillation-period and decrement were always observed, the two variations can be found separately. If we put

$$\phi = \phi_0 \cdot e^{-\frac{\lambda t}{T}} \cos\left(2\pi \frac{t}{T}\right),$$

then

$$a = \frac{\lambda}{T}, \quad b = \frac{4\pi^2 + \lambda^2}{T^2}. \quad \ldots \quad \text{(II.)}$$

Particularly, the approximate measure of the friction is therefore the quantity a, or the ratio of the decrement to the oscillation-period.

Of course the apparatus suffers also resistance by the air. This was specially observed repeatedly, but was constantly found very little in comparison with the resistance of the liquid to the motion. As the question here is not about absolute, but

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only about relative measurements, I have not thought it necessary to take account of it separately.

5. I communicate first those experiments which concern the testing of the method employed. As was remarked above, it was requisite to determine how far the oscillation-period and the logarithmic decrement are dependent on the quantity of the amplitude. The following table (Table I.) contains the results of three series of experiments. The above-described apparatus having been set oscillating, and having already executed a series of oscillations, the turnings towards both sides were noted, and therefrom the initial amplitudes determined; at the end of four oscillations the final amplitude was obtained in the same manner: both numbers are given in the first column. From them the decrement λ was calculated. At the same time the periods of the four oscillations were ascertained and the mean taken, which is returned under T. Here the above-described rectangular vessel was employed, in which the movable plates faced each other at a distance of 15 millims. The vessel was filled with distilled water, into which dipped a rectangular brass plate 100 millims. long, 5 millims. high, and 0·5 millim. thick. During series A the plate was entirely in the interior of the liquid; during B the upper edge exactly intersected the free surface, while during C it projected 0·5 millim. above. The oscillation-period T is given in seconds. The quantity λ is the difference of the ordinary logarithms.

TABLE I.

A.		
Amplitudes.	T.	λ.
424 378	11·50	0·01247
337 303	11·65	0·01155
280 252	11·45	0·01144
227 205	11·50	0·01132
185 167	11·50	0·01086
Mean ...	11·52	

B.		
Amplitudes.	T.	λ.
420 321	11·85	0·02919
353 271	11·50	0·02870
244 190	11·80	0·02716
Mean ...	11·72	

C.		
Amplitudes.	T.	λ.
502 390	12·50	0·02741
324 256	12·30	0·02556
216 172	12·70	0·02475
146 117	12·30	0·02379
Mean	12·45	

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As we see, the oscillation-period (the determination of which, as the mean of only four oscillations, was not very exact) is constant in each individual series—*i. e.* independent of the magnitude of the amplitude. It increases, when series A, B, C are compared, with the projection of the upper edge out of the free surface. There can be no doubt that this is to be ascribed to the action of attraction-forces in consequence of the curvature of the surface. The decrements slowly lessen when the amplitudes become smaller; and this diminution is pretty uniform in all the series. I have therefore not attempted to search out its more precise law, but have contented myself with taking, in all the further experiments I have to communicate, a mean value of the logarithmic decrement in each case from ten oscillations, in which the initial and final amplitudes lay in the interval from 400 to 100 scale-divisions. The great difference in the absolute values of the decrement between series A on the one hand and series B and C on the other, is to be attributed to the influence of the free surface.

I have above referred to the necessity of inclosing the liquid to be studied in a narrow vessel with parallel sides. The proof of this is given by the experiments communicated in Table II. Here also distilled water was experimented with, and the same brass plate as before employed. Under h in this, as in all the following tables, the distance of the upper edge of the plate from the free surface of the liquid is to be understood. If the edge lay below the same, and therefore within the interior of the liquid, the positive sign is employed; if it projected above, the negative.

The distance between the displaceable glass plates is indicated by the quantity e; $e=\infty$ signifies a vessel of such dimensions that any influence of the side walls was inconceivable. The oscillation-period T is given here also, in order to show the influence of the attracting forces. Lastly, the quantity $\frac{\lambda}{T}$ (which was always multiplied by 1,000,000 to avoid decimal-places) serves as the measure of the frictional resistances.

The values of the oscillation-period are in the first two series nearly constant; in the third series the attraction of the side-walls makes itself perceptible; and this influence comes out still more strongly in the last series.

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TABLE II.

h.	$\epsilon = \infty$.		$\epsilon = 30$ millims.	
	T.	$\frac{\lambda}{T}$	T.	$\frac{\lambda}{T}$
millim.				
+5	11·28	1172	11·22	1085
+0·5	11·28	1245	11·58	1119
0	11·31	1248	11·56	1632
−0·5	11·22	1222	11·36	1754
h.	$\epsilon = 20$ millims.		$\epsilon = 15$ millims.	
	T.	$\frac{\lambda}{T}$	T.	$\frac{\lambda}{T}$
millim.				
+5	11·00	1143	11·42	1188
+0·5	11·00	1277	11·44	1567
0	11·24	2355	11·82	2649
−0·5	11·84	2451	12·68	2784

The different series of experiments were made at different times; and hence, in the first place, only the numerical values of each series are comparable among themselves. Nevertheless the frictional resistances show for the motion at the greater depth ($h = 5$ millims.) a sufficiently good accordance; they are at all events not considerably influenced by the shape of the vessel. This influence, however, becomes already prominent as soon as the plate approaches near to the free surface ($h = 0{\cdot}5$ millim.), and becomes very considerable when the upper edge cuts the free surface. While in the first series the resistances remain the same, in the rest a sudden increase takes place when the plate is raised to the small extent of 0·5 millim.; and is the greater the nearer the bounding plates are. Of this great influence of the limitation of the vessel upon the resistance, the causes of which I have previously explained, I sought to convince myself in still another way.

After the resistance of a surface of pure water in the vessel in which there is no perceptible influence of the sides had been once more determined ($h = 0$), the surface was covered with a thin layer of oil spread as uniformly as possible by stirring, and the resistance again investigated. Finally, the same experiment was repeated; but onto the pure water surface a thin layer of oil of turpentine was brought in a

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similar manner. The following slightly differing values were obtained:—

Pure water surface	1248
The same with oil	1301
The same with turpentine . .	1119

while on using the narrowly separated plates ($e=15$) a scarcely visible layer of oil sufficed to make the resistance to the motion so great that the movable system returned, without oscillations, to its position of equilibrium. According to this, the use of a laterally narrowly limited vessel is found to be absolutely necessary with the oscillation-method employed.

6. This previous question being settled, I proceed to the comparison of the resistance of the interior and of the surface with different liquids. Most of the experiments were executed with the most favourable distance of 15 millims. between the plates, by means of the repeatedly mentioned brass plate of 100 millims. length; but I have not omitted to make some experiments also with the greater distance of 20 millims., as well as to use instead of the brass plate a brass cylinder of the same length and about 5 millims. in diameter.

The following Tables III., IV., V. (which are quite intelligible after the previous explanations) give only the values of $\frac{\lambda}{T}$ as measuring the resistance. For the last table I may further remark that *f* signifies a previous filtration, and that the numbers standing with the names of the liquids represent their specific gravities.

TABLE III. Rectangular brass plate.

Distance between the glass sides 20 millims.

Liquid.	$h=10$ mm.	$h=5$ mm.	$h=0{\cdot}5$ mm.	$h=0$ mm.	$h=-0{\cdot}5$ mm.
Distilled water ...	.....	1106	1310	2045	2026
Distilled water filtered	1081	1143	1277	2355	2451
Alcohol	1021	.....	1155	1024	1016

TABLE IV. Brass cylinder.

Distance between the side plates 15 millims.

Liquid.	$h=5$ mm.	$h=0{\cdot}5$ mm.	$h=0$ mm.	$h=-0{\cdot}5$ mm.
Distilled water 1	1131	1614	3388	4074
Distilled water 2	.....	1574	3301	3613
Alcohol	1331	1512	1396	1293
Oil of turpentine	1875	2327	2107	1899

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TABLE V. Rectangular brass plate.

Distance between the glass sides 15 millims.

Liquid.	$h =$ 10 mm.	$h =$ 5 mm.	$h =$ 0·5 mm.	$h =$ 0 mm.	$h =$ −0·5 mm.
Distilled water	.....	1188	1567	2649	2784
Distilled water *f*	1115	1145	1483	2545	2637
KNO_3 in water *f*, 1·1223.....	1091	1103	1444	2529	2428
Na_2SO_4 in water *f*, 1·169 ...	1499	.....	2310	4013	4088
K_2CO_3 in water *f*, 1·367.....	2175	.....	3692	5947	.....
$CaCl_2$ in water *f*, 1·348	3171	.....	5292	8155	.....
Alcohol 1	1185	.....	1463	1312	.....
Alcohol 2	.....	1225	1459	1266	1145
$CuCl_2$ in alcohol *f*, 0·878.....	1566	.....	2120	1938	.....
$CaCl_2$ in alcohol *f*, 0·995.....	6028	.....	7338	6942	6373
Sulphide of carbon	870	1144	2001	1476	1362
Oil of turpentine	2073	2276	2711	2368	2229
Water and alcohol *f*, 0·9708	1346	.....	1943	2133	.....
Water and alcohol *f*, 0·9274	1736	.....	2216	1988	1848

From the comparison of the last three Tables it is evident that the motion-resistances, in spite of the somewhat varying conditions of the experiments, show on the whole the same course. I will therefore content myself with annexing all further inferences to the numerical values of the last table. For the quicker survey of the frictional resistances, I have represented them by delineations in figures 2 and 3. For water and alcohol the means of the but little differing series of experiments have been used.

Fig. 2.

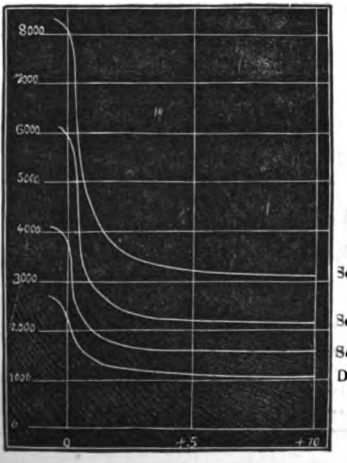

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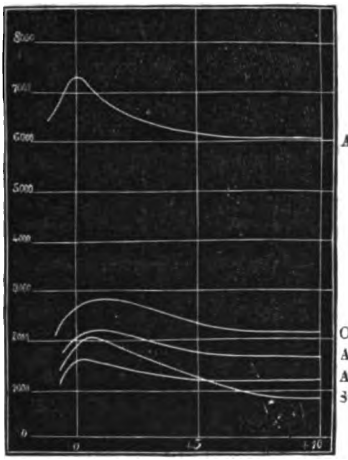
Fig. 3.



From the numerical values of the last table, as well as from these drawings, the following results are obtained:—

(*a*) In all the liquids studied, the resistance to motion is not inconsiderably augmented when the plate approaches the free surface.

(*b*) On a further elevation of the plate causing its upper edge to enter the free surface, a characteristic difference between the various liquids becomes manifest, and we find, exactly as Plateau states,

I. Liquids which exhibit a considerable augmentation of the resistance—water and aqueous salt-solutions;

II. Liquids in which a diminution of the resistance occurs —alcohol, alcoholic solutions, sulphide of carbon, oil of turpentine;

III. Mixtures of water and alcohol, which join the one or the other group according to the proportion of the constituents.

(*c*) When the resistances of the liquids at greater depths are compared, that of sulphide of carbon is the least; water and alcohol follow, with nearly equal resistances; lastly, oil of turpentine with a considerably greater resistance. The addition of a salt produces in most cases a considerable augmentation of the resistance. It is likewise to be noted as remarkable, that mixtures of water and alcohol show a greater resistance than either constituent by itself.

One portion of these results admits of a simple explanation. In the first place, that in all liquids the resistance increases as the surface is approached is not surprising; for the liquid particles put in motion by the plate give way not only in a

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horizontal direction, but also downward and upward. In close proximity to the free surface giving way in the last direction is impossible, or at least more difficult; for in this case the liquid particles would have to overstep the upper horizontal boundary plane; and with that both gravity and the capillary forces would counteract their motion. It is moreover possible that, in consequence of the evaporation taking place at the free surface, its temperature is somewhat below the mean temperature of the liquid, so that the friction, quickly changing with the temperature, is there somewhat greater than in the deeper layers. By this is probably to be explained especially the striking behaviour of sulphide of carbon, the resistance of which at greater depths is much less than those of alcohol and water, while in close proximity to the surface ($h = +0{\cdot}5$) it considerably exceeds them. In spite of the precautions which were taken, with sulphide of carbon the evaporation was so considerable that its temperature had fallen during the experiments some degrees below that of the enclosure. At all events, in these and similar experiments it is necessary to take into account the behaviour of the free surface, and therefore to bear in mind that, under some circumstances, it opposes an increased resistance to a motion taking place close beneath it.

7. To come now to the chief object of the investigation, the behaviour of the free surface, it follows unequivocally from the experiments above given that in water the resistance suddenly increases very considerably as soon as the upper edge of the plate comes into the free surface. In consequence of the entire arrangement of the experiments it appears to me beyond question that this is caused by increased friction in the surface layer. The aqueous salt-solutions studied behave very similarly to distilled water. The increase of the resistance on passing from the last position of the plate within the liquid to its entrance into the surface amounts :—

	Per cent.
In distilled water	to 60·9
In solution of KNO_3 . . .	„ 75·1
„ „ Na_2SO_4 . .	„ 73·7
„ „ K_2CO_3 . . .	„ 61·0
„ „ $CaCl_2$	„ 54·1

These numbers are, on the whole, of the same order of magnitude. The resistance in the free surface might, of course, be compared also with that at greater depths. The differences are then considerably greater still. The correctness of the facts, under the conditions of the experiments, surely cannot after this be doubted. It is true that the question thereby yet decided whether we have here to do with

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a peculiarity of homogeneous liquids, free from any foreign mixture, or whether substances coming from the atmosphere or from the sides of the vessel into the free surface cooperate. As water and salt-solutions generally possess but little different and proportionally high capillary-constants*, the possibility is given of an outspreading of substances possessing less tension.

The investigation of free water-surfaces which, not only during the experiments, but also previously, have been protected from all contact with the air might be extraordinarily difficult; at all events the arrangements necessary for it were wanting to me.

The precautions employed by me—filtering the water (heated to boiling) into a narrow-necked flask, covering the glass vessel during the experiment with wet filtering-paper—had almost no influence at all upon the numerical values obobtained. Also water from the conduit did not show any essential difference from distilled water. I tried still another means in order to ascertain the action of the contact of the air or of the particles suspended in it. If that is the chief reason for the peculiar behaviour of the water, the action must increase with the time which has elapsed since the free surface was formed. Respecting this I made the following experiments. As quickly as possible after pouring the liquid into the vessel the state of the free surface was examined; after a longer time the examination was repeated. Finally it was attempted by mechanical means (stirring with a clean platinum plate, shortly before heated to incandescence) to remove foreign bodies from the surface. Some of these experiments are collected in the following table. The numbers given are, as before, the quotients $\frac{\lambda}{T}$. The brass plate, as well as the brass cylinder, were placed so that their upper extremity exactly touched the free surface.

TABLE VI.

Rectangular plate: distance betwen the sides 20 mm.		Rectangular plate: distance between the sides 15 mm.		Cylinder; distance between the sides 15 mm.	
h. m.		h. m.		h. m.	
1 20	2045	11 7	2649	9 30	3388
2 45	2132	12 5	2744	10 45	3360
After stirring	1909	After 24 hours	∞	Stirred	3374
		Stirred	2539		

* Quincke, Pogg. *Ann.* clx. p. 337–375, 560–588 (1877).

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The first two series show a small increase of the resistance in the interval of an hour; in the last series it remains constant. After a period of 24 hours the resistance has become so great that it could not be determined by the method employed. After stirring, however, it falls back to only a little less than the initial value. Although, therefore, it cannot be disputed that longer contact with the air increases the resistance, just as little can it be doubted that a special surface-resistance is present immediately after the formation of the free surface. Hence we must conclude either that a very great surface-resistance belongs to the free water-surface, or that a pure water-surface in contact with the air has no existence at all. We should then have, in the method of observation employed, a fine means of judging of the condition of a water-surface.

While in water and aqueous salt-solutions the increase of resistance at the surface is very considerable, the decrease of the same in the other liquids is proportionally small. It amounts in

	Per cent:
Alcohol	to 11·9
Alcohol solution of $CuCl_2$.	„ 8·6
„ „ $CaCl_2$.	„ 5·4
Oil of turpentine	„ 12·6
Sulphide of carbon . . .	„ 26·3

The behaviour of the last liquid is (as was previously found to be the case) abnormal in consequence of the strong evaporation; so that a proportionally large diminution cannot be surprising. In the rest of the liquids, moreover, the resistance at the surface (contact with it by the upper edge of the plate) is always still greater than on the motion of the plate at a greater distance from the free surface. Hence I do not think we are entitled to infer from these experiments a diminution of the coefficient of friction at the surface.

In solutions of water in alcohol we get:—

At the sp. gr. 0·9708, increase 9·8 per cent.
„ „ 0·9274, decrease 10·3 „

From this it follows that even a trifling addition of alcohol to water considerably lessens its characteristic property, while on a further addition the mixture behaves nearly like pure alcohol. This appears to me to stand in the closest connexion with the rapid diminution of the cohesion of water by the addition of alcohol. For it Quincke* gives the following values:—

* Pogg. *Ann.* clx. p. 368 (1877).

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σ	0·9973	0·9852	0·9110	0·7904
α	8·000	5·657	2·947	2·354

where σ denotes the specific gravities, and α the cohesion-constants.

In order to see on what the resistances to the motion of the plate in the interior of the liquid chiefly depend, I determined, with the aid of an oscillating brass disk, the coefficients of friction of some of the liquids employed, according to the formulæ and rules given by O. E. Meyer*. They are collected in the following table, in which I have confined myself to giving their relative values referred to water. The temperature amounted to 21° C.; only with sulphide of carbon was it lower, and that in consequence of the hardly to be avoided evaporation. Since the absolute values of η for distilled water,

According to O. E. Meyer†, at 21°·6 0·01190,
According to Grotrian‡, at 21°·5 0·01250, at 21°·58 0·01236,

are known, the friction-coefficients of the other liquids can easily be calculated from the following table.

TABLE VII.

	ρ.	$\eta . \rho$.	η.	$\frac{\lambda}{T}$
Sulphide of carbon ...	1·293	0·4262	0·3297	0·782
Oil of turpentine	0·870	2·030	2·333	1·859
Alcohol	0·7937	1·055	1·329	1·063
„	0·8720	2·282	2·617	
„	0·9023	2·720	3·014	
„	0·9737	1·721	1·767	
Water.....	1·000	1·000	1·000	1·000

A comparison of the values of $\eta . \rho$ with the ratios of the resistances $\frac{\lambda}{T}$ shows, as was to be expected, no perfect agreement. Much rather, in the resistances the specific gravity of the liquid has also some influence. The peculiar behaviour of the mixtures alcohol and water, which possess a maximum of η, has moreover been already observed by Poiseuille§.

Halle a. d. S., Aug. 1, 1880.

* Pogg. *Ann.* cxiii. 1861.
† Ibid. cxiii. p. 399.
‡ Ibid. clx. p. 242 (1877).
§ Ibid. lviii. p. 437 (1843).

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XX. *Notices respecting New Books.*

Elements of Astronomy. By Robert Stawell Ball, *LL.D., F.R.S., Royal Astronomer of Ireland.* 459 pages, with 136 woodcuts. Longmans and Co.

OF the text-books of Science published by Longmans none surpass, and we question whether any equal, this in the essentials of an elementary text-book, namely, clearness of diction, simplicity of exposition, abundance and excellence of geometrical figures and illustrations. These features, in our opinion, make this the text-book *par excellence* for beginners. The author of a work on Astronomy naturally presupposes a certain amount of mathematical knowledge on the part of his readers, but nothing more than a sixth-form boy would in the ordinary course of things possess. Even without this knowledge, there is sufficient to instruct and interest the general reader. We readily and cordially agree with Mr. Ball, that the book is adapted for the use of students in Public and Science Schools; but that it will be adopted we very much doubt. For, owing to some reason best known to Head Masters, Astronomy has never been permitted to take root in Public Schools as part of the curriculum. It is only of late years that the desire for scientific instruction has been properly recognized. Thousands of young pupils spend several hours a week for years together over Euclid without either interest or profit. This is, no doubt, in a measure due to the stiff and formal language of the text-book mostly used, and the unnecessary and wearisome length of the demonstrations; but it is still more due to the repulsive form in which it is set before them by their teachers, who, often being mere repeaters, have not the power to awaken an interest in others in a subject in which they take no interest themselves. No such complaints can reasonably be made with respect to Mr. Ball's book. An hour or two a week given to it could not fail to interest any class of intelligent young people, and, we know from personal experience, might give a fillip to many a sluggish mind.

The book consists of twelve chapters, which are divided into numbered paragraphs; and the order and treatment of the subject throughout are thoroughly scientific. The first three chapters treat of Astronomical Instruments, the Earth, and the diurnal motion of the Celestial bodies. Chapters IV., V., and VI. are devoted exclusively to the Sun and Moon. Unusually well-drawn woodcuts illustrate this part of the work. Chapters VII. to XI. deal with the Planets, Comets, Universal Gravitation, Stars and Nebulæ, and the Structure of the Sun. To those already acquainted with Astronomy the book will be mainly prized for chapter XII., which is on Astronomical Constants, consisting of eighty-eight pages of matter, comprising the most recent discoveries and a vast amount of valuable bibliographical information, directing students who wish to pursue the subject where to lay their hands on the original sources. The book is in every sense creditable both to the author and publishers; and we trust it will receive the success it so well deserves.

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Elementary Plane Geometry. By JULIUS PETERSEN. *Translated into English by* R. STEENBERG, Jun. (*Authorized English Edition.*) Sampson Low, Marston, Searle and Rivington. 72 pages.

Considering the great number of elementary books on Plane Geometry which have been published during the past twelve years (many better and few worse than the one now before us), the *raison d'être* of an English edition of M. Petersen's work is not very apparent. It consists of 72 pages, and contains 228 exercises, the latter being unquestionably the best part of the book, certainly as regards quality, if not quantity. Could the accomplished and amusing author of 'Euclid and his Modern Rivals' have seen it before publishing his work, he would certainly have found material for his piquant criticism.

Congruent figures are badly defined, if defined at all in the true sense of a definition; and the sign for *congruence* is simply grotesque. He, too, would be a sharp boy indeed who could understand that the exterior angles of any convex polygon are together equal to four right angles from the explanation given in art. 16, or make out what is meant by "*parallels all over are at the same distance from each other.*"

The translation is undoubtedly creditable to a foreigner; but we would suggest to the author and translator the advisability of getting an Englishman to correct the proofs of the next edition; for the present contains from beginning to end many very unenglish expressions. Just twelve years ago a well-known 'Athenæum' Reviewer (Jan. 9, 1869, No. 2150) wrote, "After a score of attempts to overthrow Euclid, we shall have a chance of a good attempt to amend the 'Elements.'" We have seen the score attempts exceeded; but the chance of getting a book worthy of supplanting Euclid seems as far off as ever.

Mathematical Formulæ. Edited by R. M. MILBURN, *M.A.* Longmans and Co. 100 pages.

The title does not do the book full justice. We opened it expecting to find formulæ only, but found a number of definitions which are models of clearness and succinctness. Besides tables of Squares and Cubes and their reciprocals of numbers from 1 to 100, there are excellent rules and definitions in Commercial Arithmetic. In Algebra, comprising ratio, proportion, variation, and permutations, the definitions are perfect and the formulæ all that could be desired. The same may be said of every other branch of Mathematics. Besides this system of *memoria technica*, several well-executed figures add to the value of the book. We repeat, the title is defective; "Definitions, Rules, Hints, and Formulæ" would be more appropriate. The book is only a compilation; but it is manifestly the work of an excellent judge of the subjects treated, as the various definitions and hints throughout the book testify.

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Elementary Algebra, with brief notices of its History. *By* ROBERT POTTS, *M.A., Trin. Coll. Camb.* In twelve sections, demy 8vo, pp. 618. Messrs. Longman and Co.

In this important treatise on Algebra Mr. Potts has bestowed much thought, judgment, and research on a very instructive and interesting History of the subject, in which he traces its rise, growth, and progress from the time of Euclid and Diophantus to the present time. The first twelve sections are entirely devoted to it, and comprise references to the works of the most distinguished writers on Algebra in every country where the subject has been encouraged. The various notes and quotations are skilfully compressed; and it may be conceded that Mr. Potts has earned the gratitude of teachers who take more than an ordinary interest in this subject.

The arrangement of the subsequent sections is excellent: clear and copious explanations of principles, carefully selected exercises, results, and hints—these form the order of the different sections. The *rule of signs* is made perfectly plain; and *factorial expressions* are treated exhaustively. On turning to the section on Simple Equations, we were glad to find that the traditional *fish* and *post* questions have no place there. There are no "*clock*" questions and others of a like class, all of which are more suitable to the pages of an Arithmetic. But, on the other hand, we have an unusual number of problems which will exercise the reasoning faculty of young pupils. The exercises generally, which are chiefly taken from Cambridge Examination Papers, will prove a mine of wealth for examination-purposes.

The scope of the book is somewhat greater than that of ordinary text-books, as it includes (and concludes with) a section on Cubic and Biquadratic Equations. It may be said that Mr. Potts has now done for Algebra what he did for Geometry many years ago, and in our opinion has done it much better. It is a sequel to his work on Arithmetic; and the two, together with the edition of Euclid alluded to, form a complete and valuable répertoire of a student's requirements in Elementary Mathematics.

On the Motion of a Solid in a Fluid, and the Vibrations of Liquid Spheroids. *By* THOMAS CRAIG, *Ph.D., Fellow in Physics in the Johns Hopkins University, Baltimore, Ind. Van Nostrand's Science Series.* London: Trübner & Co.

This small treatise is the 49th of a series of text-books on Science, mostly on Mechanical and Civil Engineering and cognate subjects. The differential equations of fluid-motion are simplified, in the author's opinion, by the introduction of curvilinear in the place of rectangular coordinates. This transformation being accomplished he then proceeds to investigate the subject in question. Of the seven sections into which the book is divided, section 3, "*On the flow of a fluid through an orifice*," is very ably written. Many valuable references to the highest authorities on the subject are

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interspersed throughout the book, and the author's sources of information duly acknowledged. But, owing to the abstruse nature of the subject, this treatise only addresses itself to the learned in Mathematics, and will for the most part be perfectly unintelligible to the ordinary student of science.

XXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

January 5, 1881.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Archæan Geology of Anglesey." By C. Callaway, Esq., M.A., D.Sc., F.G.S. With a Note on the Microscopic Structure of some of the Rocks, by Prof. T. G. Bonney, M.A., F.R.S., Sec.G.S.

The author discussed the stratigraphy and lithological characters of the rocks in the following areas:—The border of the Menai Strait, the Llangefni region, and the central zone about Bodafon, Llangwyllog, Llanerchymedd, and Paris Mountain, which, he considers, establish the following conclusions:—(1) that in Anglesey there are two Archæan groups, the slaty and the gneissic; (2) the slaty is composed of slates, shales, limestones, grits, conglomerates, and chloritic schists, in which at present a definite order has not been ascertained (the gneissic group is composed of the following, in *descending* order—granitoidite, chloritic and hornblendic schists, grey gneiss, quartz-schist, and hälleflinta); (3) the slaty series is occasionally foliated, but is usually in a partially altered state; the gneissic group is thoroughly metamorphosed; (4) the slaty series has closer lithological affinities with the St.-David's volcanic group, the Charnwood rocks, and the Lilleshall series than with the Bangor group; (5) the slaty series is undoubtedly Pebidian, the gneissic series may, with some probability, be referred to the Dimetian. The microscopic structure of the principal varieties of the rocks mentioned in the above paper was described by Prof. Bonney.

2. "The Limestone of Durness and Assynt." By C. Callaway, Esq., D.Sc., F.G.S.

This paper gave the result of an examination of the vicinity of Durness and Inchnadamf, where Lower-Silurian fossils occur in a limestone, as discovered by Mr. C. Peach. At Durness the only evidence of the limestone underlying the schist is the asserted fact of the dip being in the same direction; for all admit the junction to be a faulted one. The author showed that while the flaggy (upper)

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schists dip uniformly to N.E., the limestone dips in a very variable manner E.S.E., E., and but rarely N.E., any dip N. of E. being exceptional and then only at a distance from the schist. Again, the Smoo mass of limestone, cut off from the Durness area by a faulted strip of gneiss, dips either E.S.E., or even more to S. After discussing the relation of the quartzite and gneisses, the author passed to the Assynt district, and pointed out that the relations of the limestone and the quartzite are by no means satisfactorily established, that their conformity is rendered dubious by a marked discordance of strike, and that the limestone lies in a synclinal basin, so that its dip in one place is in the opposite direction to that of the quartzite. From the above considerations the author holds that in these districts there is no proof of the Lower-Silurian age of the quartzite and newer series of flaggy gneiss and schist.

3. "On a Boulder of Hornblende-Pikrite near Pen-y-Carnisiog, Anglesey." By Prof. T. G. Bonney, M.A., F.R.S., Sec.G.S.

The boulder described had been originally about a cubic yard in volume; and the fragments lay in a field to the left of the road from Pen-y-Carnisiog to Bwlyn. The ground-mass consists of hornblende and serpentinous products, with a little mica. In this are crystals, often ¼ inch long, of brown hornblende with enclosures of altered olivine. The author doubted whether this hornblende is not a paramorph after augite; some of that in the ground-mass is certainly of secondary origin. He compared the rock with a pikrite from the Lleyn peninsula and two described by Prof. Geikie from Fifeshire. It differs from all these, but has a singular resemblance to a pikrite from Schriesheim (Odenwald), except that it is rather more altered. He called attention to the rock in hopes that some geologists may discover it *in situ*, as it will be of much value in deciding in what direction the ice has moved over Anglesey.

January 19, 1881.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Further Notes on the Family Diastoporidæ, Busk." By G. R. Vine, Esq. Communicated by Prof. P. Martin Duncan, M.B. Lond., F.R.S., F.G.S.

2. "Further Notes on the Carboniferous Fenestellidæ." By G. W. Shrubsole, Esq., F.G.S.

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XXII. *Intelligence and Miscellaneous Articles.*

ON THE MOTION OF AN INCOMPRESSIBLE VISCOUS FLUID IN THE VICINITY OF A SPHERE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Halle a. S., Dec. 29, 1880.

IN the November number of the Philosophical Magazine (vol. x. No. 63), pp. 342–357, is a paper by Dr. Thomas Craig, of Washington, communicated by A. G. Greenhill, Esq., "On Steady Motion in an Incompressible Viscous Fluid." The second part of that paper treats of the motion of the fluid in the vicinity of a sphere. *In the year* 1875, in Borchardt's *Journal für reine und angewandte Mathematik*, vol. lxxxi., I discussed the same question: I place at your disposal the enclosed copy of my paper. You will easily convince yourselves, by the perusal of § 2 (pp. 68–74) of the same, that:—

(1) Mr. Craig and I have set and discussed exactly the same problem;

(2) There is a close correspondence between the transformations and calculations in the one paper and in the other (I mention especially that Dr. Craig likewise makes use of a theorem of Borchardt's, *Monatsberichte der Berl. Akad.* 1873, in the same way as I had done);

(3) Of course our final results are identical.

I therefore venture to hope that you will give as early insertion as possible to this in the Philosophical Magazine.

Dr. A. OBERBECK, Professor at the University of Halle a. d. Saale.

ON THE MEASUREMENT OF THE ELECTROMOTIVE FORCE OF THE PILE. BY M. J.-B. BAILLE.

The relative electromotive force of the pile is usually deduced from Ohm's laws and the examination of the intensity of the currents produced by it. When one wishes to measure these forces directly and to express them in absolute value, the very ingenious apparatus of Sir W. Thomson is ordinarily employed; but these instruments require delicate management, and the indications given by them are not always free from doubt. The study which M. Cornu and I have made of the torsion-balance for our experiments

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on the density of the earth led me to think that this apparatus, the construction and regulation of which are so simple, might be put in action by the weakest forces and give very precise measurements.

The precautions necessary for obtaining good oscillations consist in avoiding some perturbations which present themselves, always the same, and the relative influence of which would be the greater the more delicate the phenomenon to be studied.

Among those perturbations, M. Cornu and I have already pointed out electric influences and trepidations of the ground. These are completely eliminated by surrounding the apparatus with a metallic case attached to the ground, and setting it up in a very quiet place upon solid pillars.

I have been exposed to another very serious disturbing cause —the influence of heat. It makes itself sensible upon the torsion-thread, and upon the air of the case which surrounds the lever. These two effects are easily distinguished from one another. When the thread alone is heated or cooled, the oscillations are quite irregular and disordered; if the air alone is subjected to the action of the heat, the oscillations remain nearly pendular, but the point of static equilibrium about which the oscillation takes place is more or less quickly displaced, always in the same direction, while the duration of an oscillation is slightly increased. I protected myself from these very tenacious perturbing influences by surrounding the apparatus with a thick layer of wood-shavings.

The apparatus which I employed was composed of a long torsion-thread (270 centims.) of annealed silver, and a lever 50 centims. in length, carrying at each end a ball of gilt copper of 3 centims. diameter. Similar spheres were fixed to the vertices of a rectangle of 20·5 metres, and communicated with each other in pairs diagonally. The lever, placed at equal distance from the fixed spheres, communicated, through the intermedium of the torsion-thread, with the positive pole of a determined pile P, the other pole being to earth.

The charge of the lever was not so constant as I could have desired; for piles always undergo variations difficult to define. I was also obliged to measure this charge at each observation. I connected the positive pole of the pile P simultaneously with the lever and a pair of fixed spheres, and took the resulting deflection; I recommenced by taking the deflection on the other side, and so on alternately four times.

The pile to be measured, X, was composed of ten equal elements; and I put one of the poles of this pile into communication with the fixed balls, while the lever remained in contact with P, and the other pole of X was to earth; then I measured the charge of the lever a second time.

The deflections were read by means of the reflection of an illuminated glass scale placed at 3·5 metres from the lever. All the elements of the measurement were known; and Coulomb's static

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formula gave

$$C\alpha = \frac{4mm'}{d^3}$$

with

$$O = \sqrt{\frac{\Sigma \frac{p}{g} r^2}{C}}.$$

In my experiments $O = 437^s$, and $\Sigma pr^2 = 32171{\cdot}6$ centimetre-grammes, the lever having been made of a geometrical form.

The following numbers represent the potential of an element of the pile—that is to say, the quantity of electricity which would be shed by the pole of that pile upon a sphere of 1 centim. radius; they are expressed in electric units, the unit being the quantity of electricity which, acting upon itself at 1 centim. distance, produces a repulsion equal to 1 gramme.

Voltaic pile	0·03415,	circuit open.
Pile (zinc, sulphate of copper, copper)	0·02997,	„
„ (zinc, acidulated water, copper, sulphate of copper)	0·03709,	„
„ (zinc, salt water, carbon, peroxide of manganese).....	0·05282,	„
„ (zinc, salt water, platinum, chloride of platinum)	0·05027,	„
„ (zinc, acidulated water, carbon, nitric acid)	0·06285,	„

These numbers are maxima values, obtained at the moment when the pile had just been charged; but these potentials rapidly grow less as the pile gets older. The sulphate-of-copper pile alone remains for a pretty long time near the given number; but it undergoes variations which may reach one twelfth of its value, more or less.—*Comptes Rendus de l'Acad. des Sciences*, Jan. 3, 1881, t. xcii. pp. 32–34.

A STUDY ON SPECTROPHOTOMETERS. BY M. A. CROVA.

The comparison of two spectra proceeding from two different sources of light is easily obtained by covering half the slit of a photometric spectroscope with a small rectangular prism, of which the edge cuts the slit normally into two parts. One half receives one of the lights directly, the other by total reflection the other light placed laterally. In this case the slit must be horizontal; and if the prism is well cut and free from aberration, the clean image of the spectral lines and that of the edge of the prism are obtained simultaneously—the latter appearing as a very fine line separating the two spectra to be compared from one another.

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In the most general case Sturm's theorem leads to the conclusion that all the rays which constitute an extremely delicate pencil, reflected or refracted a certain number of times by plane surfaces, meet two indefinitely small straight lines contained in two rectangular planes.

Excepting the case of the minimum of deviation, therefore, a simple prism always gives an aberration which can be represented by that of a cylindrical lens, convergent or divergent according to the case, the axis of which is parallel to the refracting edge; this aberration can be corrected by means of a cylindrical lens suitably arranged. Direct-vision prisms are often affected by an aberration of the same kind, so that clean images can only be obtained in two directions, parallel or perpendicular to the refracting edge, by different degrees of drawing-out of the telescope of the spectroscope*.

I ascertained this aberration by substituting for the slit a reticule formed of two rectangular strokes traced on a coat of silver and illuminated by monochromatic light. I have been able to correct it by intercalating between the dispersing-prism and the telescope a system of two cylindrical lenses, plano-convex and plano-concave, of the same curvature: if the two curved parts are in contact, the system constitutes a medium with parallel faces; on being separated to a suitable distance they operate like a cylindrical lens whose focal length, variable at pleasure, can be calculated as a function of the distance between the two lenses.

The employment of prisms with total reflection introduces, into the polarized ray which they reflect, a difference of phase between the two principal components, and gives at the emergence an elliptically polarized ray. I have measured this difference of phase in the apparatus I use; the ellipticity produced by it may detract from the accuracy of the photometric measurements obtained by means of a rotating Nicol. This elliptical polarization by total reflection can be completely suppressed by substituting for the simple prism two prisms with total reflection, superposed, in contact, whose two right sections are rectangular, or a single prism suitably cut, producing the effect of the two prisms of which I have just spoken.

With this disposition, the slit may be placed vertical, which is more convenient. After the two total reflections undergone by the light in this prism, the plane of polarization has rotated 90°, and all difference of phase between the two principal components has vanished, the reflected ray preserves the rectilinear polarization.

These principles can be utilized in the construction of spectrophotometers, and, I expect, will permit more rigorous measurements to be obtained.—*Comptes Rendus de l'Académie des Sciences*, 1881, No. 1, t. xcii. pp. 36, 37.

* A prism affected with such an aberration would give images of the solar protuberances sharp in one direction only; the measurements of their heights might therefore be affected with an error if the aberration of the prism were not suitably corrected.

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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MARCH 1881.

XXIII. *On the Phosphorograph of a Solar Spectrum, and on the Lines in its Infra-red Region.* By JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York**.

I PROPOSE in this communication to consider (1) the peculiarities of a phosphorograph of the solar spectrum as compared with a photograph of the same object; (2) the antagonization of effect of rays of higher by those of lower refrangibility.

There is a striking resemblance between a photograph of that spectrum taken on iodide of silver and a phosphorograph taken on luminous paint and other phosphorescent preparations. There are also differences.

I. *Description of the Photographic Spectrum.*

In 1842 I obtained some very fine impressions of the first kind (on iodide of silver), and described them in the Philosophical Magazine, November 1842, and again in February 1847. One of these was made the subject of an elaborate examination by Sir J. Herschel. His description and explanatory views of it may be found in that journal, February 1843.

From these it appears that such a photograph, *taken in presence of a weak extraneous light*, may be considered as pre-

* Read before the American Academy of Arts and Sciences, and communicated by the Author.

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senting three regions :—1. A middle one, extending from the boundary of the blue and green to a little beyond the violet; in this region the argentic iodide is blackened. 2. Below this, and extending from the boundary of the blue and green to the inferior theoretical limit of the prismatic spectrum, is a region strongly marked, in which the action of the daylight has been altogether arrested or removed, the daylight and the sunlight having apparently counterbalanced and checked each other. 3. A similar protected region occurs beyond the violet; this, however, is very much shorter than the preceding. The sketch annexed to Herschel's paper represents these facts as well as they can be by an uncoloured drawing.

II. *Description of the Phosphorographic Spectrum.*

In a phosphorograph on luminous paint the same general effects appear. If the impression of the spectrum be taken in the absence of extraneous light, there is a shining region corresponding to the blackened region of the photograph. But if, previously or simultaneously, extraneous light be permitted to be present, new effects appear. The shining region of the phosphorograph has annexed to it, in the direction of the less-refrangible spaces and extending toward the theoretical limit of the spectrum, a region of blackness in striking contrast to the surrounding luminous surface. This blackness, however, is broken at a distance below the red by a luminous rectangle of considerable width. This occupies the space, and indeed arises from the coalescence, of the bands a, β, γ, discovered by me in 1842. It may be separated into its constituent bands, which are very discernible when registered on gelatine as presently described. And since this is not so easily done with the upper lines of the spectrum, we may infer that these are very much broader than the Fraunhofer lines, a result strengthened by the fact that these dark intervals can be more easily recognized by the thermopile than those lines. The blackness is then resumed. It extends to a short distance; and there the phosphorographic impression comes to an end.

This shining rectangle has long been known to students of phosphorescence; but its interesting origin has not until now been explained.

But more, just beyond the region of the violet, the same kind of action occurs—a dark space, which, however, is of very much less extent than that beyond the red.

The photograph and the phosphorograph thus present many points of similarity. But though there are these striking points of resemblance, there are also striking differences.

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In a spectrum four or five centimetres long, though the photograph may be crossed by hundreds of Fraunhofer lines, not one is to be seen in the phosphorograph, except those just referred to. The spectrum must be dispersed much more before they can be discerned.

III. *Of the Propagation of Phosphorescence from Particle to Particle.*

The explanation of this disappearance of the Fraunhofer lines is obvious. A phosphorescing particle may emit light enough to cause others in its neighbourhood to shine; and each of these in its turn may excite others; and so the luminosity may spread. In a former memoir I examined this in the case of chlorophane, and concluded that in that substance such a communication does not take place; but now, using more sensitive preparations, as follows, I have established in a satisfactory manner that it does.

The test plate referred to in the next parapraph was thus made. A piece of glass was smoked on one side in a flame, until it became quite opaque. When cool a few letters or words were written on it. Some photographic varnish was poured on it and drained. This, drying quickly, gave a black surface which could be handled without injury.

A phosphorographic tablet was made to shine by exposure to the sky. It was then carried into a dark room, and the test plate laid upon it. On the test plate another non-shining phosphorographic tablet was laid, and kept in that position a few minutes; then, on lifting this from the test plate, the letters were plainly visible, especially if it were laid on a piece of hot metal. So the light radiating from the first tablet through the letters of the test could produce phosphorescence in the second tablet, through glass more than a millimetre thick.

This lateral illumination is therefore sufficient to destroy the impression that is left by the fixed lines, unless indeed their breadth be sufficiently exaggerated and as short an interval as possible permitted between the moment of insolation and that of observation.

It has been remarked that a photograph taken from a phosphorograph is never sharp. It looks as if it were taken out of focus, and this even though it may be a copy by contact. The light has spread from particle to particle. Under such circumstances, sharpness is impossible, because the phosphorograph itself is not sharp.

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For this reason also, the bright rectangle in a phosphorograph of the solar spectrum, arising from the coalescence of the infra-red lines α, β, γ, is never sharp on its edges. It seems as if it were fading away on either side. It is also broader than would correspond to the actual position and width of those lines; and, particularly, it is somewhat rounded at its corners.

If we could obtain a thermograph of the solar spectrum, it would correspond very closely to the phosphorograph. The particles heated would radiate their heat to adjacent ones. Nothing like sharpness of definition could be obtained, except in very brief exposures before the effect had had time to spread.

IV. *Examination of Phosphorescent Tablets by Gelatine Photography.*

The examination of a phosphorescent surface can be made now in a much more satisfactory manner than formerly. The light we have to deal with, being variable, declines from the moment of excitation to the moment of observation; and, though the phosphori now prepared are much more sensitive and persistent than those formerly made, they must still be looked upon as ephemeral. To examine them properly, the eye must have been a long time in darkness to acquire full sensitiveness.

It was recommended by Dufay to place a bandage over one eye, that its sensitiveness might not be disturbed, whilst the other, being left naked, could be used in making the necessary preparations. But this on trial will be found, though occasionally useful, on the whole an uncomfortable and unsatisfactory method.

The exceedingly sensitive gelatine-plates now obtainable remove these difficulties. The light emitted by blue phosphori, such as luminous paint, consists largely of rays between H and G; and these are rays which act at a maximum on the gelatine preparation. So if a gelatine plate be laid on a shining blue phosphorus it is powerfully affected, and any mark or image that may have been impressed on the phosphorus will on development in any of the usual ways be found on the gelatine. The gelatine has no need to wait after the manner of the eye. It sees the phosphorus instantly. It is impressed from the very first moment; and whilst the eye is accommodating itself and so losing the best of the effect, the gelatine is gathering every ray and losing nothing. Moreover the effect upon it is cumulative. The eye is

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affected by the intensity of the emitted light, the gelatine by its quantity. Each moment adds to the effect of the preceding. The gelatine absorbs all the light that the phosphorus emits from the moment of excitation, or by suitable arrangement any fractional part thereof. It has another most important advantage. The phosphorus is yielding an ephemeral result, and is momentarily hastening to extinction; so that for a comparison of such a result with others of a like kind the memory must be trusted to. But the gelatine seizes it at any predetermined instant, and keeps it for ever. These permanent representations can at any future time be deliberately compared with one another.

To these still another advantage may be added. Very frequently an impression is much more perceptible on a gelatine copy than it is on the phosphorus from which that copy was taken. This arises from the fact that the eye is made less sensitive by the light emitted from surrounding phosphorescent parts, and cannot perceive a sombre point or line amongst them. This is a physiological effect. But a gelatine copy in no respect dazzles or enfeebles the eye. For this reason, for instance, we may not be able in a phosphorograph to resolve visually the infra-red bright rectangle into its constituent lines, but we recognize them instantly in the gelatine.

I have made use of sensitive gelatine plates ever since their quality of being affected by phosphorescent light was announced by Messrs. Warnecke and Darwin. The more sensitive of the plates receives a full effect by an exposure of less than one minute.

But all kinds of phosphori will not thus affect a photographic tablet: there must be a sympathy between the phosphorescent and the photographic surfaces. Thus a phosphorus emitting a yellow light will not affect a photographic preparation which requires blue or indigo rays. This principle I detected many years ago. In my memoir on phosphorescence (Phil. Mag., February 1851), it will be seen that the green light emitted by chlorophane could not change the most sensitive photographic preparation at that time known (the daguerreotype plate); and hence I was obliged, in measuring the light it emits, to resort to Bouguer's optical method. The result would have turned out differently had the light to be measured been more refrangible, blue or indigo or violet.

A photographic surface agrees with the retina in this, that it has limits of sensitiveness. The eye is insensible to rays of much lower refrangibility than A and much higher than

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H. Gelatine cannot perceive rays lower than F; but it is affected by others far higher than H. There is therefore a range for each, having its limits and also its place or point of maximum sensitiveness. But some substances, such as the iodide and bromoiodide of silver, under special methods of treatment, are either affected positively or negatively throughout the entire range of the spectrum.

In experiments for obtaining quantitative results, it should be borne in mind that there is generally a loss of effect. Between the moment of insolation and that of perception, either by the eye or by gelatine, emitted light escapes. The moment of maximum emission is the moment of completed insolation; and from this the light rapidly declines. It is necessary, therefore, to make that interval between the two moments as short as possible.

V. *Of the Extinction of Phosphorescence by Red Light.*

I turn now to an examination of those parts of the phosphorographic spectrum from which the light has been removed. They are from the line F to the end of the infra-red space, and again for a short distance above the violet. The effect resembles the protecting action in the same region of a photograph.

Now, if similar effects are to be attributed to similar causes, we should expect to find in the photograph and phosphorograph the manifestation of a common action.

Several different explanations of the facts have been offered. Herschel suggested that the photograph might be interpreted on the optical principle of the colours of thin films. Very recently Captain Abney has attributed the appearance of the lower space to oxidation. But this can scarcely be the case in all instances. Mr. Claudet showed, in a very interesting paper on the action of red light, that a daguerreotype plate can be used again and again by the aid of a red glass, and that the sensitive film undergoes no chemical change (Phil. Mag., February 1848).

It was known to the earliest experimenters on the subject that if the temperature of a phosphorescent surface be raised, the liberation of its light is hastened, and it more quickly relapses into the dark condition. In the memoir to which I have previously referred (Phil. Mag., February 1851), I examined minutely into this effect of heat, and determined the conditions which regulate it. And since, on the old view of the constitution of the solar spectrum, the heat was supposed

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to increase toward the red ray, and when flint-glass or rock salt-prisms are employed to give its maximum far beyond that ray, it was supposed that this heat expelled the light, and consequently in all those parts of the phosphorus on which it fell the surface became dark through the expulsion or exhaustion of the light.

I speak of this as "the old view," because, as I have elsewhere shown, the curve supposed to represent heat, light, and actinism, so called, have in reality nothing to do with those principles. They are merely dispersion-curves having relation to the optical action of the prism and to the character of the surface on which the ray falls. (Phil. Mag., August 1872, December 1872.)

But this heat explanation of the phosphorescent facts cannot be applied to the photographic. Nothing in the way of hastened or secondary radiation seems to take place in that case.

In phosphorescence the facts observed in the production of this blackness are these. If a shining phosphorescent surface be caused suddenly to receive a solar spectrum, it will instantly become brighter in the region of the less-refrangible rays, as will plainly appear on the spectrum being for a moment extinguished by shutting off the light that comes into the dark room to form it. If the light be re-admitted again and again, the like increase of brilliancy may again and again be observed, but in a declining way. Presently, however, the region that has thus emitted its light begins to turn darker than the surrounding luminous parts. If now we no longer admit any spectrum-light, but watch the phosphorescent surface as its luminosity slowly declines, the region that has thus shot forth its radiation becomes darker and darker, and at a certain time quite black. The surrounding parts in the course of some hours slowly overtake it, emitting the same quantity of light that had previously been expelled from it; and eventually all becomes dark.

Now, apparently, all this is in accordance with the hypothesis of the expulsion of the light by heat. There are, however, certain other facts which throw doubt on the correctness of that explanation.

On that hypothesis, the darkening ought to begin at the place of maximum heat—that is, when flint-glass apparatus is used, below the red ray; and from this it should become less and less intense in the more refrangible direction. But in many experiments, carefully made, I have found that the maximum of blackness has its place of origin above the line D, and, indeed, where the orange and green rays touch each

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other. Not unfrequently, in certain experiments the exact conditions of which I do not know and cannot always reproduce, the darkening begins at the upper confines of the green, and slowly passes down to beyond the red extremity; that is to say, its propagation is in the opposite direction to that which it ought to show on the heat hypothesis.

Still more, as has been stated, there is a dark space above the violet. Now it is commonly held that in this region there is little or no heat. If so, what is it that has expelled or destroyed the light.

The experiments above referred to I made with the recently introduced luminous paint. It presented the facts under their simplest form. But I have also tried many other samples, for which I am indebted to the courtesy of Professor Barker of Philadelphia. Among them I may mention as being very well known the specimens made by Dubosc, enclosed in flat glass tubes, contained in a mahogany case, and designed for illustrating the different-coloured phosphorescent lights emitted. They are to be found in most physical cabinets. These, however, do not show the facts in so clear a manner. On receiving the impress of a solar spectrum they present patches of light and shade irregularly distributed. Though in a general way they confirm the statements made above, they do not do it sharply or satisfactorily.

Dubosc's specimens to which I have had access are enumerated as follows:—1. Calcium violet; 2. Calcium blue; 3. Calcium green; 4. Strontium green; 5. Strontium yellow; 6. Calcium orange. Restricting my observation to the space beyond the red (which, as has been said, presents a bright rectangle in the darkness, about as far below the red as the red is below the yellow) I found that this rectangle is not given by 1 and 2; in 3 it is doubtful; in 4 it is quite visible; and in 5 and 6 strikingly so.

Is the blackening then due to heat? That it occurs beyond the violet (that is, beyond the lines H) seems to render such an opinion doubtful; for it is commonly thought that the effect of heat is not recognizable there. And in the phosphorogenic spectroscope I have used, the optical train (prism, lenses, &c.) is of glass, which must of course exercise a special selective heat-absorption; but the traces of this in the phosphorograph I could never detect.

In the diffraction-spectrum, I had attempted nearly forty years ago to ascertain the distribution of heat (Phil. Mag., March 1857), but could not succeed with the experiment in a completely satisfactory manner, so small is the effect. I exposed a tablet of luminous paint to such a diffraction-spec-

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trum formed by a reflecting grating having 17,296 lines to the inch, and was not a little surprised to see that from the blue to the red end of the spectrum there is an energetic extinction of the light, and darkness is produced. I repeated this with other gratings, and under varied circumstances, and always found the same effect.

Now, considering the exceedingly small amount of heat available in this case, and considering the intensity of the effect, is there not herein an indication that we must attribute this result to some other than a calorific cause?

I endeavoured to obtain better information on this point by using the rays of the moon, which, as is well known, are very deficient in heating-power. Many years ago I had obtained some phosphorographs of that object. With the more sensitive preparations now acessible, and with a telescope of 11 inches aperture and 150 inches focus, there was no difficulty in procuring specimens about 1·4 inch in diameter. These represented the lunar surface satisfactorily. At half-moon an exposure of three or four seconds was sufficient to give a fair proof. But, on insolating a phosphorescent tablet, and causing the converging moon-rays to pass through the red glass which I commonly use as an extinguisher, no effect was produced by the red moonlight on the shining surface.

I repeated this experiment, using a lens 5 inches in diameter and 7 inches focus, so arranged that the moon's image could be kept stationary on the phosphorescent tablet. That image was about $\frac{1}{3}$ inch in diameter. Then, insolating the tablet, the moon-rays, after passing through a red glass, were caused to fall upon it. The exposure continued ten minutes; but no effect was produced on the shining surface. The lunar image was so brilliant that when the red glass was removed, and a non-shining phosphorescent surface was exposed to it, a bright image could be produced in a single second.

But in order to remove the effect of the more refrangible rays by the less, the latter must not only have the proper wave-length but also the proper amplitude of vibration. This principle applies both to photographic and phosphorographic experiments. In my memoir on the negative or protecting rays of the sun (Phil. Mag., February 1847) it is said, "Before a perfect neutralization of action between two rays ensues, those rays must be adjusted in intensity to each other." It requires a powerful yellow ray to antagonize a feeble day-light.

It is owing to the difference in amplitude of vibration that the heat of radiation seems so much more effective than the heat of conduction. A temperature answering to that of the

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boiling-point of mercury must be applied to a phosphorescent tablet for quite a considerable time before all the light is extinguished. But the red end of the spectrum and that even of the diffraction-spectrum, in which the heat can with difficulty be detected by the most sensitive thermometer, accomplishes it very quickly.

VI. *On the Infra-red Lines or Bands in the Sun's Spectrum.*

At a distance about as far below the red as the red is below the yellow in the solar spectrum, I found in 1842, in photographs taken on iodide of silver (Daguerre's preparation), three great lines or bands, with doubtful indications of a fourth still further off. I designated them as α, β, γ, and published an engraving of them in the Philosophical Magazine for May 1843.

In 1846, MM. Foucault and Fizeau having repeated the experiment thus originally made by me, presented a communication to the French Academy of Sciences. They had observed the antagonizing action above referred to, and had seen the infra-spectral lines α, β, γ. They had taken the precaution to deposit with the Academy a sealed envelope, containing an account of their discovery, not knowing that it had been made and published long previously in America.

Sir J. Herschel had made some investigations on the distribution of heat in the spectrum, using paper blackened on one side and moistened with alcohol on the other. He obtained a series of spots or patches, commencing above the yellow and extending beyond the red. Some writers on this subject have considered that these observations imply a discovery of the lines α, β, γ. They forget, however, that Herschel did not use a slit, but the image of the Sun, an image which was more than a quarter of an inch in diameter. Under such circumstances, it was impossible that these or any other of the fixed lines could be seen.

I have many times repeated this experiment, but could not obtain the same result, and therefore attributed my want of success to unskilfulness. More recently Lord Rayleigh (Phil. Mag., November 1877), having experimented in the same direction, seems to be disposed to attribute these images to a misleading action of the prism employed. Whatever their cause may be, it is clear that they have nothing to do with the fixed lines α, β, γ now under consideration.

In these experiments, and also in others made about the same time on the distribution of heat in the spectrum, I attempted to form a diffraction-spectrum without the use of any dioptric

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media, endeavouring to get rid of all the disturbances which arise through the absorptive action of glass by using as the grating a polished surface of steel on which lines had been ruled with a diamond, and employing a concave mirror instead of an achromatic lens; and though my results were imperfect and incomplete, I saw enough to convince me that it is absolutely necessary to employ a spectrum that has been formed by reflection alone. (Phil. Mag., March 1857, p. 155.)

In 1871, M. Lamanski succeeded in detecting these lines or bands by the aid of a thermomultiplier. He was not adequately informed on what had already been done in the matter in America; for he says that, "with the exception of Foucault and Fizeau in their well-known experiments on the interference of heat, no one as yet has made reference to these lines." Nearly thirty years before the date of this memoir I had published an engraving of them (Phil. Mag., May 1843).

After I had discovered these three lines, I intended to use the grating for the exploration of that region, since it extends it far more than the prism can do—but on making the attempt was discouraged by the difficulty of getting rid of the more refrangible lines belonging to the second spectrum. I had hoped to eliminate these by passing the ray on its approach to the slit through a solution of the bichromate of potash. But the bichromate in long exposures permits a sufficiency of the more refrangible rays to pass, to produce a marked photographic effect; and hence I feared that any experiments supposed to prove the existence of lines in the infra-red would be open to the criticism that they in reality belonged to the more refrangible region of the spectrum of the second order, and that a satisfactory examination of the case would exclude the use of the grating and compel that of the prism. With the prism I could not obtain clear evidence of the existence of more than three lines, or perhaps groups, and doubtful indications of a fourth. If in these examinations we go as far as wave-length 10,750, the limit of Captain Abney's map, we nearly reach the line H^2 of the third spectrum. This would include all the innumerable lines of spectrum 2, and even many of those of spectrum 3. In such a vast multitude of lines, how would it be possible to identify those that properly belonged to the first, and exclude those of the second and third spectra? Besides, do we not encounter the objection that this is altogether beyond the theoretical limit of the prismatic spectrum?

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This brings us to Captain Abney's recent researches, which, by the aid of the grating, carry the investigation referred to the prismatic spectrum as far below the red as the red is below the yellow. They are not to be regarded as an extension of exploration in the infra-red region (for they really do not carry us beyond my own observations in 1843), but as securing the resolution of these lines or bands into their constituent elements. I had never regarded them as really single lines; the breadth or massiveness of their photographs too plainly suggests that they are composed of many associated ones. The principle of decreasing refrangibility with increasing wave-length incapacitates the prism from separating them; but the grating (which spreads them out according to their wave-length) reveals at once their composite character.

In Captain Abney's map, after leaving the red line A, we find three groups:—(1) ranging from about 8150 to 8350; (2) from 8930 to 9300; (3) from 9350 to 9800. These, admitting that the lines of the subsequent grating-spectra have been excluded, are then the resolution of a, β, γ.

I suppose that care has been taken to make sure of that, either by absorbent media or by a subsidiary prism. If the grating had been ruled in such a manner as to extinguish the second spectrum, inconveniences would arise from the characteristics thereby impressed on the first.

In the phosphorographic spectrum on luminous paint, this vast multitude of lines is blended into a mass which probably can never be completely resolved into its elements, on account of the propagation of phosphorescence from particle to particle. I have resolved it into two or three constituent groups, and frequently have seen indications of its capability of resolution into lines, in the serrated aspect of its lateral edges.

I believe that luminous paint enables us to approach very nearly, if not completely, to the theoretical limit of the prismatic spectrum.

The history of these interesting infra-red lines is briefly this. They were discovered by me in 1842, and an engraving and description of them given in the 'Philosophical Magazine.' They were next seen by Foucault and Fizeau in 1846, and a description of them presented to the French Academy of Sciences. They were again detected by Lamanski with the thermopile in 1871. Their resolution into a great number of finer lines was accomplished by Abney, who gave a Bakerian lecture describing them before the Royal Society in 1880. Finally they have been redetected by me

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in the shining rectangle, just above the theoretical limit of the prismatic spectrum, given by many phosphorescent substances.

University of New York,
Dec. 1, 1880.

XXIV. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part III.* *By* C. R. ALDER WRIGHT, *D.Sc.* (*Lond.*), *Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School*†.

On the Absolute Values of the Ohm and of the Mechanical Equivalent of Heat, and on the Nature of Polarization and Electrolysis, and the amount of work done when the latter occurs.

55. SINCE the publication of Parts I. and II. of these researches, it has been obligingly pointed out by Mr. L. B. Fletcher, of Baltimore (Phil. Mag. [5] x. p. 436), that, by a slip, it is stated in § 35 that the values of the B.A. unit of resistance deducible from the 1867 experiments of Joule and from those detailed in Part II., are from 1·5 to 2·0 per cent. *above* one earth-quadrant per second, instead of that amount *below* this theoretical value, it being assumed that the mean water-friction value of Joule (41·555 megalergs, § 34) is correct; and Mr. Fletcher also states that the first results of an experimental redetermination of the absolute value of the ohm, on which he is now engaged, are similar in character. Besides the valuations of Lorenz and Kohlrausch, briefly referred to in Part I. as illustrations of the want of absolute certainty as to the true value of the B.A. unit, two other sets of observations on this point have been made within the last two or three years—one by Prof. H. A. Rowland (Silliman's Journal [3] xv. pp. 281, 325, and 430, 1878), and the other by H. F. Weber (Phil. Mag. [5] v. pp. 30, 127, and 189, 1878). By making observations of the value of the current induced in one coil by the establishment of a measured current in another one, Rowland obtained values of the resistance of the former such that, by comparison with four copies of the B.A. unit (or a multiple thereof), the following numbers were obtained as the resistance of the latter:—

* Part I., Proceedings of the Physical Society, 1880, vol. iii. p. 213; also Phil. Mag. April 1880. Part II., Proc. Phys. Soc. 1880, vol. iii. p. 246; also Phil. Mag. May 1880.

† Communicated by the Physical Society.

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		Earth-quadrant per second.
Coil No. 1.	B.A. unit, by Elliot . . .	0·99257
„ 2.	10-ohm coil, „ . . .	0·98963
„ 3.	10-ohm coil, by Warden, Murhead, and Clark . . .	0·99129
„ 4.	100 „ „ „	0·99098
	Mean	0·99112

Hence, on the whole, Rowland's experiment indicates an error in the B.A. unit of 0·00888=0·888 per cent. *in deficiency.*

Weber's determinations, on the other hand, indicate that the B.A. unit is nearly correct, the slight error being one of excess, and that the value of J is somewhat above that deduced from Joule's water-friction experiments. Thus he obtained the following values for J in gravitation measure; the annexed values in ergs are calculated on the supposition that at Zurich the value of g is 981·5.

	Gravitation measure.	Megalergs.
Value derived from calculations based on the different specific heats of a gas, from experimental determinations by Regnault and Röntgen	428·95	42·10
Value derived from experiments by H. F. Weber, on the heat developed by a known current whilst passing through a resistance measured in absolute measure by himself	428·15	42·02

Taking the mean of these values for J (=428·55 in gravitation measure), and determining the heat produced by a measured current in a circuit the resistance of which was measured by himself in absolute units, Weber obtained a mean value for the Siemens unit of resistance of $0{\cdot}9550 \times 10^9$ C.G.S. units, employing a coil (No. 1914) obtained from W. Siemens as standard. By two other different methods (one depending on the determination of the rate of oscillation of a magnetic needle in a coil, the other on the measurement of the current induced in a given coil by opening a known current in an adjacent circuit) the resistance of this same standard coil was found to be respectively $0{\cdot}9545 \times 10^9$ and $0{\cdot}9554 \times 10^9$, giving as final average of the three series of observations, $0{\cdot}9550 \times 10^9$ C.G.S. units. Unfortunately Weber did not compare his standard Siemens unit with any copy of the B.A. unit: assuming, however, that the Siemens unit used by him bore

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to the B.A. unit the same ratio as that found by Kohlrausch (§ 36), viz. 1 to 1·0493, the value of the B.A. unit becomes finally

$$0{\cdot}9550 \times 1{\cdot}0493 = 1{\cdot}0021 \text{ earth-quadrant per second,}$$

exhibiting an error in excess of 0·0021 = 0·21 per cent.

Taking these results into consideration along with those of Kohlrausch and Lorenz, discussed in Part II., the following values are finally deducible for the Siemens and B.A. resistance units respectively in earth-quadrants per second:—

	Siemens unit.	B.A. unit.
B.A. Committee	0·9530 (*a*)	1·0000
Kohlrausch	0·9717	1·0196
Lorenz	0·9337	0·9797 (*b*)
Rowland	0·9446 (*a*)	0·99112
H. F. Weber, 1st method . .	0·9550	1·0021 (*b*)
2nd „ . . .	0·9545	1·0016 (*b*)
3rd „ . . .	0·9554	1·0025 (*b*)

the values marked (*a*) for the Siemens unit being derived from the observed value of the B.A. unit by dividing by 1·0493, the ratio found by Kohlrausch, and those marked (*b*) for the B.A. unit being obtained by multiplying the observed values of the Siemens unit by 1·0493. Mr. Fletcher's preliminary results as above stated give a value less than 1·0000. Taking all these valuations into consideration, it would seem that, in the existing state of our knowledge, it is just about as probable that the B.A. unit is in error in deficiency as that it is erroneous in excess. Accordingly, in the following experiments it is assumed that the B.A. unit represents exactly one earth-quadrant per second, and in consequence that the E.M.F. of a Clark's cell is exactly, as determined by him, $1{\cdot}457 \times 10^8$ C.G.S. units, or 1·457 theoretical volt.

56. Admitting this to be so, however, it does not seem to be possible that Joule's water-friction values of J are exact (§ 34): not only do the determinations of Hirn and Violle concur in indicating values for J of from nearly 2 to 3 per cent. higher than these water-friction values (§ 37), but, further, four different sets of observations also indicate that if the B.A. resistance-unit be correct (*i. e.* if it = 1 earth-quadrant per second), or if the Siemens unit be 0·9530 earth-quadrant per second, the value of J must be from 1 to 2 per cent. higher than these values, and, on the whole, upwards of 42 megalergs. These observations are, first, those of von Quintus Icilius (Pogg. *Ann.* ci. p. 65) as corrected by H. F. Weber (*loc. cit. suprà*), which were based on an erroneous

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early determination of the absolute value of a Jacobi resistance-unit by W. Weber; after correction they indicate a value for J of 431·6 in gravitation measure, or, assuming $g=981$, of 42·34 megalergs; and, next, those of Joule himself, those of H. F. Weber above quoted, and those of the author and Mr. Rennie (Part II.), which yield the following results:—

	Value of J, the B.A. unit being supposed to be exactly 1 earth-quadrant per second.
Von Quintus Icilius (corrected by H. F. Weber)	about 42·34 megalergs.
Joule (1867, *vide* § 33), the results of which investigation have been shown to be too low (§ 54) .	above 42·10 „
H. F. Weber*, $42{\cdot}02 \times \frac{1{\cdot}0000}{1{\cdot}0021}$ . .	41·93 „
Alder Wright and Rennie (value slightly too low)	above 41·96 „

Moreover, as stated above, H. F. Weber has calculated, from Regnault's and Röntgen's experiments on the specific heats of air &c., that J=42·10 megalergs. Hence, finally, taking into account all the above experiments, the most probable value of J lies close to 42 megalergs, being above rather than below that value. Until further authoritative determinations finally settle the question as to the true values of the B.A. unit and of J, the former may, as above stated, be considered to be exactly 1 earth-quadrant per second, and the latter may be safely taken as being 42 megalergs, most probably lying between 41·75 and 42·25 megalergs. In the following parts of of this paper, consequently, J is taken $=42\times10^6$ C.G.S. units, $\alpha\chi J$, the factor for reducing gramme-degrees to E.M.F. units (volts), is taken as being $=0{\cdot}000105\times42\times10^6=4410{\cdot}0$.

General Conclusions from the results of previous experimenters, and those subsequently detailed: Statement of Theorem relative to Polarization and the Nature of Electrolysis and the Amount of Work done therein.

57. It has been shown in Part I. (§§ 13–17) that the work expended in decomposing an electrolyte into the "nascent" products of electrolysis is not necessarily the same as that performed in breaking it up into the final products of electrolysis, and that by determining (in terms of E.M.F. or other-

* Weber's result refers to air-thermometer temperatures, those of the other experimenters to temperatures measured by the mercurial thermometer and not reduced to air-thermometer readings.

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wise) the two quantities, conclusions may be drawn as to the general nature of the phenomena connected with the "nascent state." If the difference of potential between the electrodes of an electrolytic cell be E, during the passage of a quantity of electricity Q sufficient to decompose a gramme of electrolyte in a time t (thereby giving rise to a current $C=\frac{Q}{t}$) a total amount of work $EQ=ECt$ is done, of which $C^2Rt=CQR$ represents the work done as heat due to the resistance of the cell R (Joule's law, Part I. §§ 1 & 2); consequently an amount $EQ-C^2Rt=(E-CR)Q$ of work is done in decomposing the electrolyte into the nascent products of decomposition. The E.M.F. E − CR representing this amount of work is readily measurable by electrical means; for it is the quantity sometimes spoken of as the "counter E.M.F. set up during electrolysis," being the value of e in Ohm's theorem

$$C=\frac{\Sigma(E)-e}{\Sigma(R)+R},$$

where $\Sigma(E)$ is the sum of the electromotive forces of the battery, e the counter E.M.F. set up, $\Sigma(R)$ the total resistance in circuit less that of the electrolytic cell, and C the current produced. For if r be the "resistance of a wire obstructing the current equally with the electrolytic cell" (*i. e.* what may be conveniently termed the *apparent* resistance of the voltameter, equal to that of a wire through which the current C would pass when a difference of potential E subsisted between its ends), we have

$$C=\frac{E}{r}=\frac{\Sigma(E)-e}{\Sigma(R)+R}=\frac{\Sigma(E)}{\Sigma(R)+r};$$

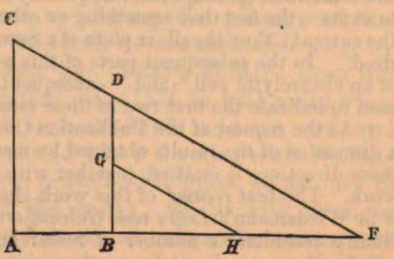
whence*

$$C \text{ also } =\frac{E}{r-R}=\frac{\Sigma(E)-E}{\Sigma(R)}=\frac{E-e}{R},$$

and

$$e=E-CR.$$

* These relationships may be represented graphically thus:—On a base-line A B H F let fall perpendiculars A C, B D, representing respectively $\Sigma(E)$ and E, the length A B representing $\Sigma(R)$. Join C D, and produce the line C D till it cuts A B produced in F. Cut off from B D at the end D a length D G representing e, and through G draw G H



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58. Very many experiments have been made during the last thirty or forty years on the values of e under varying conditions, on a somewhat closely allied thing, viz. the E.M.F. existent between the electrodes of an electrolytic cell *after rupture of the current**, and on the E.M.F. of gas-batteries (and especially those with hydrogen and oxygen as gases)†. From these researches it results that, other things being equal, the counter E.M.F. set up during electrolysis increases with the current-strength as the size of the electrodes is diminished and as the temperature is lowered, at least within certain limits. The observations made have almost invariably been performed with currents of moderate or considerable magnitude; with such currents in no case has a value for e been obtained less than the E.M.F. corresponding to the decomposition into the final products, whilst with powerful currents much larger values have been obtained: thus with water, all values of e registered lie above 1·50 volt, and sometimes exceed 3 volts (when reduced to that unit). Certain experiments of Andrews (Phil. Mag. [3] x. p. 433), and Buff (*Ann. Chem. Pharm.* xciv. p. 1), made by decomposing water with a feeble battery-power (less than 1·50 volt), appear, however, to show that a less value for e may exist when a very minute current only is employed, in which case simultaneous evolution of hydrogen and oxygen does not take place; if the one or the other be prevented from

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parallel to C D F, cutting A F in H. Then B F represents r, and B H represents R. And by similar triangles,

$$\frac{AC}{AF}=\frac{AC-BD}{AB}=\frac{AC-DG}{AH}=\frac{BD}{BF}=\frac{BG}{BH}=\frac{DG}{HF},$$

or

$$\frac{\Sigma(E)}{\Sigma(R)+r}=\frac{\Sigma(E)-E}{\Sigma(R)}=\frac{\Sigma(E)-e}{\Sigma(R)+R}=\frac{E}{r}=\frac{E-e}{R}=\frac{e}{r-R}.$$

* The term "polarization" is sometimes applied to mean the counter E.M.F. of an electrolytic cell e as above defined, sometimes the E.M.F. set up between the electrodes after the current has ceased to flow (a quantity always less than the value of e existent whilst the current passes), and sometimes (perhaps most frequently of all in a vague undefined sense), to express the fact that something or other takes place tending to diminish the current: thus the silver plate of a Smee's cell is said to become "polarized." In the subsequent parts of this paper the terms "counter E.M.F. of an electrolytic cell" and "subsequent polarization" respectively are used to indicate the first two of these meanings.

† At the request of the Publication Committee of the Physical Society a discussion of the results obtained by many previous observers bearing in these directions is omitted, together with the numerous references to their work. The best *résumé* of this work that the writer is acquainted with is in Wiedemann's *Lehre vom Galvanismus*, which, however, does not include a considerable number of researches published within the last few years.

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making its appearance as a free gas by using a large electrode and a minute one (Wollaston's point), a feeble evolution of gas may be noticed (especially at first) from the small electrode, but none from the other. Similarly, the experiments of Helmholtz on what he terms "electrolytic convection" (Phil. Mag. [4] xlvii. p. 152), and analogous observations of others, show that, if the one or the other gas is suppressed by the chemical action of other kinds of gas respectively dissolved in the fluid surrounding the electrode or adherent to the electrode surface, water may be decomposed by an E.M.F. of less than 1·50 volt.

59. As regards "subsequent polarization," or the E.M.F. existent between the electrodes of an electrolytic cell after the original current has been ruptured, even when measured by a quick-working switch or "wippe," it results from the observations at present on record that a more or less perceptibly smaller value is always obtained than corresponds to the counter E.M.F. set up whilst the current was passing. Under certain circumstances, especially when currents of short duration are employed, the difference becomes very marked; so that on the whole "subsequent polarizations" have been recorded, for acidulated water for instance, of magnitude varying from something upwards of two or even three volts to something less than ·001 volt. With currents producing smaller values than 1·5 volt (measured immediately after rupture) visible evolution of gas at *both* poles does not seem to have ever been noticed. Recently Exner (Wied. *Annalen*, vi. p. 336, 1870) has shown that to cause visible evolution of gas from boiled-out distilled water, a "subsequent polarization" (measured immediately after rupture by a quadrant electrometer) of from 2·03 to 2·09 Daniell cells (from 2·2 to 2·3 volts) must be established; whilst if the water be faintly acidulated, a polarization of about 1·89 Daniell = 2·1 volts is sufficient. And in the same paper he has also shown that the polarization set up after the current is ruptured differs from the E.M.F. of the battery employed (and *à fortiori* from the counter E.M.F. set up during the passage of the current) only by quantities too small to measure accurately by a quadrant electrometer, when the battery E.M.F. does not exceed from 1·40 to 1·47 Daniell cell (1·54 to 1·61 volt) when larger platinum plates and boiled-out dilute acid are used, and from 1·94 to 2·09 Daniell cells (2·1 to 2·3 volts) with Wollaston's points (platinum wires coated with glass, so as only to expose a minute surface at the ends); whilst with stronger battery electromotive forces than these values the subsequent polarization always falls sensibly short of the battery E.M.F.

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60. Various determinations have been made of the E.M.F. which a Grove's gas-battery can generate under various conditions. When the gases are oxygen and hydrogen, it has uniformly been found that the E.M.F. generated is less than 1·50 volt, the value representing the work done in the union of ordinary gaseous oxygen and hydrogen to form liquid water, which is the end result of the action taking place in such a cell when it generates a current. Recently Osgood Pierce (Wied. *Annalen*, viii. p. 98, 1879) has found numbers varying from ·766 to ·926 Daniell cell (·84 to 1·02 volt), according to the temperature, the nature of the liquid present, the purity of the oxygen, &c. *Cæteris paribus*, increasing the temperatures lowers the E.M.F; Morley has recently shown (Proceedings Physical Society, ii. p. 212, 1878) that when a Grove gas-battery produces a current the E.M.F. is not constant, but is a function of the current produced, being the smaller the greater the current, *i. e.* the less the external resistance, and *vice versa*.

61. A clear coherent explanation of the differences in the numerical values of these quantities obtainable under various conditions, accounting quantitatively for them, and consistent with well-known dynamical principles, does not appear as yet to have been brought prominently forward; the following attempt in that direction is the result of a somewhat prolonged experimental investigation, some of the results of which are detailed subsequently.

Theorem.—Were it possible that the surface-action of the electrodes, the chemical action of substances dissolved in the fluid electrolysed, or of the electrode materials themselves, and other interfering causes (such as the accumulation of fluids of different densities round the electrodes) could be entirely eliminated, *the primary effect of electrolysis would be to break up the electrolyte into "nascent" products, the formation of which would correspond to a definite amount of work, and consequently to a definite E.M.F.*; but the physical attraction exerted by the electrodes upon the nascent products causes a certain proportion of them (variable with circumstances) to be converted *ab initio* into products, the formation of which from the "nascent" products is accompanied by a gain of energy (or evolution of heat). Similarly, under certain conditions the products of electrolysis are chemically acted upon by the electrodes, or by gases occluded upon their surfaces, substances dissolved in the fluid electrolysed, &c.; whilst, as the action proceeds, solutions of different densities accumulate round the electrodes, tending to set up an E.M.F. in virtue of their interdiffusion. All these circumstances modify the

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amount of work actually performed by the curent whilst causing electrolysis, in such a way that *the algebraic sum of the energy gained by these other actions is subtracted from the energy that would otherwise be required to break up the electrolyte into nascent products.*

This theorem may be put in the form of the following equation.—Suppose that the quantity of total products of electrolysis at the negative electrode be called 1, and out of this amount let $1-n_1$ parts be evolved "nascent," whilst n_1 parts are so affected by the condensing or attractive action of the electrode as to be evolved *ab initio* not in the "nascent" condition, but in a more condensed form, viz. in the ordinary physical condition of the substance, if non-gaseous, but, if naturally gaseous, in the form of a more or less condensed form of gaseous matter (occluded gas, quasi-liquefied gas, or gas strongly attracted to the surface of the electrode). Let H_1 be the heat evolved per gramme-equivalent of product in its transformation from the "nascent" to the ordinary free condition, and let h_1 be the heat evolved in further condensing a gramme-equivalent of the substance to the more condensed form in which the fraction n_1 of total product is evolved. Similarly let n_2, H_2, and h_2 be the values corresponding respectively to n_1, H_1, and h_1, referring to the products at the + electrode taken as unity. Then the amount of energy to be subtracted from that corresponding to the decomposition into the nascent products due to the attractive action of the electrodes is given in terms of E.M.F. by the expression

$$\{n_1(H_1+h_1)+n_2(H_2+h_2)\}\chi J.$$

The energy corresponding to the work done in the decomposition into nascent products being expressed in terms of E.M.F. by E_l, let $\Sigma(\mathbf{H})$ represent the heat-evolution per gramme-equivalent of substance decomposed, due to the algebraic sum of the chemical actions taking place between the products and the electrodes, &c., and of the diffusive action of the differently constituted liquids surrounding the two electrodes; then the final E.M.F. corresponding to the work done in electrolysis will be

$$e=E_l-\{n_1(H_1+h_1)+n_2(H_2+h_2)+\Sigma(\mathbf{H})\}\chi J.$$

If now E_1 represent the E.M.F. corresponding to the work done in decomposing the electrolyte into the final products, supposing them to escape in the ordinary free condition,

$$E_1=E_l-(H_1+H_2)\chi J:$$

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whence

$$e=E_1+\{(1-n_1)H_1-n_1\,h_1+(1-n_2)H_2-n_2h_2-\Sigma(\mathbf{H})\}\chi J,$$

which may for shortness be written

$$e=E_1+[\Sigma\{(1-n)H\}-\Sigma(nh)-\Sigma(\mathbf{H})]\chi J.$$

62. According to the particular values which the several terms in this expression may have, various values for e may be deduced. On examination it is found that all the apparently abnormal variations in the value of the counter E.M.F. set up during electrolysis, of the subsequent polarization, and of the closely allied E.M.F. produced by gas-batteries and analogous combinations may be satisfactorily accounted for by this expression *. Thus, for instance, suppose water to be electrolysed with platinum poles, so that no chemical action takes place between the electrodes and the products, whilst no suppression of hydrogen by the action of dissolved oxygen, or of hydrogen by dissolved oxygen takes place; then $\Sigma(\mathbf{H})=0$. If now the electrode be supposed to be destitute of adherent or occluded gases to commence with, and the current be very minute, the whole of the water decomposed will be transformed into hydrogen and oxygen in a highly condensed form, whence, since $n_1=1$ and $n_2=1$,

$$e=E_1-(h_1+h_2)\chi J=E_1-\Sigma(h)\chi J.$$

Under such conditions therefore *the water will be electrolysed by an* E.M.F. *less than* 1·50 *volt* (the value of E_1), Experiment shows in fact that when well-boiled water (either nearly pure or acidulated with sulphuric acid) is decomposed with recently ignited platinum electrodes, an E.M.F. scarcely appreciably greater than 0 will suffice to enable a current to pass, whence $(h_1+h_2)\chi J$ must, under these conditions, nearly $=1{\cdot}50$ volt,

$$\text{or } h_1+h_2 \text{ must nearly } =34{,}100 \text{ gramme-degrees};$$

that is, *the heats of condensation of oxygen and hydrogen by platinum &c. jointly may amount to almost as large a quantity as that developed by their union to form liquid water.*

That the work done in the condensation of a film of gas by attraction to or occlusion in a solid body is great relatively to the mass of gas condensed is well known. The Author, con-

* At the request of the Publication Committee of the Physical Society, the discussion of several particular cases of this theorem and of their bearing upon and explanation of various phenomena observed by previous experimenters is omitted.

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jointly with Prof. W. C. Roberts, has shown (Chem. Soc. Journal, 1873, p. 112) that the specific heat of the portion of hydrogen first occluded by palladium is much greater than that of the later portions, the limiting values being near to 9 on the one side and 3·4 on the other—or values respectively representing a condensation to something approaching the liquid state (judged by analogy with liquid bromine, the product of the combining number and specific heat of which is near 9), and a condensation but little exceeding that due to ordinary atmospheric pressure. Presumably when a film of gas is attracted to the surface of a solid, the portions first attracted are very highly condensed, the next ones less so, and so on, a kind of atmosphere or aura surrounding the body being thus produced of density varying from the maximum amount of condensation to the minimum, *i. e.* the pressure of the surrounding atmosphere. The total quantity of gas thus condensed is not inconsiderable: thus an ordinary porcelain or, better still, a platinum crucible, if ignited and cooled in dry air, will weigh less by from one to several milligrammes, according to its size, if weighed immediately it is cold, and before the film or aura of condensed gas expelled by the heat is completely re-formed, than it will weigh if allowed to remain some time in the dry air, and weighed after the aura has been attracted to saturation. The inequality in the deviations from Boyle's law exhibited by gases under high pressures, but when still far removed from their condensing or from their critical points, has been considered by many physicists to be probably largely due to the difference in the attracting or aura-forming capacity exhibited by the tubes and measuring-vessels used, according as the nature of the gas varies. Not only gases, but also liquids, are attracted by solids, forming a layer or aura of highly condensed liquid adherent to the solid: Schliermacher has recently calculated (Dingler, *Pol. Journal*, pp. 224, 471; Chemical-Society Journal, Abstracts, 1880, p. 363) that this causes an appreciable error in the determinations of the weight of a unit volume of water hitherto made.

63. When water is electrolysed with non-oxidizable recently ignited electrodes (*e.g.* of platinum), the length of time which must elapse before the electrodes become saturated with gases will depend on circumstances, and amongst others on the fact that the water will tend to take up oxygen at the + electrode and hydrogen at the − electrode, and the solutions thus produced will diffuse towards the opposite poles, thus tending to set up a double form of "electrolytic convection."

Suppose that a current pass through acidulated water, so that gases are slowly evolved; if by saturating with oxygen

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the water surrounding the — electrode the hydrogen is wholly reconverted into water, leaving out of sight the formation of liquids of differing densities round the two electrodes, and supposing that the + electrode and fluid surrounding it are saturated with oxygen, it will result that

$$\Sigma(\mathbf{H}) = E_1 + \{(1-n_1)H_1 - n_1 h_1\}\chi J$$

(the n_1, H_1, and h_1, referring to hydrogen), whence

$$e = \{(1-n_2)H_2 - n_2 h_2\}\chi J.$$

Now when $n_2 = 0$ this value is +, being $H_2\chi J$; but if $n_2 = 1$ it would become negative, $= -h_2\chi J$: for certain values of n_2 therefore (*i. e.* for certain rates of current-flow, Part IV.), the value of e whilst still + must be less than 1·50 volt; *i. e.* when "electrolytic convection" takes place, so that the hydrogen evolved by electrolysis is reoxidized as fast as generated, *electrolysis may be produced by means of a less E.M.F. than* 1·50 *volt.* Obviously the same result follows if the hydrogen be unoxidized, but the oxygen be reconverted into water by dissolved hydrogen; and the same may ensue if only a part of the hydrogen or oxygen is thus reconverted into water.

64. Again, when $\Sigma(\mathbf{H})$ is negligible compared with the other terms, and n_1 and n_2 are both small, the value of e may be greatly superior to E_l; for when n_1 and n_2 are both $=0$ and Σ (H) is negligible,

$$e = E_1 + (H_1 + H_2)\chi J = E_l.$$

Experiment shows that, *cæteris paribus*, the stronger the current the smaller are the values of n_1 and n_2; that is, e continually increases as the current increases, tending towards a limiting value in any given case (Crova, *Ann. Chim. et Phys.* lxviii. p. 413, 1868). Although at present the data for calculating accurately the limiting values of E_l for various substances are not extant, yet it is known that in the case of water E_l must be upwards of 3 volts; for values of e above 3 volts have been observed: that is, the value of $H_1 + H_2$ is greater than 34,100 gramme-degrees; or *the heat of transformation of the "nascent" products of electrolysis of water into gaseous oxygen and hydrogen jointly exceeds the heat developed by the combination of these gases at ordinary temperature and pressure to form liquid water.*

The theorem therefore accounts for all possible variations in the counter E.M.F. set up during electrolysis, between the minimum values (when the normally gaseous products of electrolysis are evolved not as free gases, but as condensed

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films of attracted or occluded gas, or when they are chemically acted upon by the electrodes or other substances in contact with them, *e. g.* during the so-called "electrolytic convection" of Hemholtz) and the limiting maximum reached with an indefinitely large current.

65. Before passing from this point it is worth noticing that the circumstance that the limiting value of E_1 for water must lie above 3 volts shows that the first action of the current can be neither of the three following changes :—

(1) $2H_2O = H_2O_2 + H_2$;

(2) $3H_2O = 3H_2 + O_3$;

(3) $\begin{cases} H_2O + H_2SO_4 = H_2 + H_2SO_5 \\ \text{or } H_2O + 2H_2SO_4 = H_2 + H_4S_2O_9, \end{cases}$

forming respectively gaseous hydrogen and hydrogen dioxide, gaseous hydrogen and ozone, and gaseous hydrogen and the "persulphuric acid" of Berthelot; for that observer has shown (*Bulletin Soc. Chim. Paris*, 1876, xxvi. p. 56, and 1880, xxxiii. p. 246), that the amounts of heat absorbed by the addition of 8 grammes of oxygen to 9 of water to form hydrogen dioxide, to 16 of oxygen forming ozone, and to sulphuric acid forming persulphuric acid, are respectively 10,800, 14,800, and 13,800 gramme-degrees; so that the transformation of 8 grammes of ordinary oxygen into 8 of ozone would absorb $\frac{14,800}{3} = 4933$ gramme-degrees, and the above three decompositions would absorb per gramme-equivalent (1 gramme of hydrogen evolved) respectively $34,100 + 10,800 = 44,900$, $34,100 + 4933 = 39,033$, and $34,100 + 13,800 = 47,900$ gramme-degrees, corresponding to the setting-up of counter electromotive forces amounting respectively to only 1·98, 1·72, and 2·11 volts, or far below the actual maximum. Even, therefore, if it be admitted that persulphuric acid is the body of which the oxygen that finally escapes is first produced as a constituent, it must be supposed that the hydrogen is evolved as an allotropic modification absorbing heat in its formation from ordinary gaseous hydrogen; whilst if this be admitted for hydrogen, it seems at least probable that the same is true for oxygen, and that the hydrogen dioxide, persulphuric acid, and ozone, found to be formed under certain conditions, are secondary products due to the reaction of the "nascent" oxygen on water or sulphuric acid, or to its "rearrangement" into ozone, just as the oxygen ultimately developed is due to a further rearrangement.

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In accordance with the ordinarily received views as to the atomic and molecular constitution of matter, it would seem to result from the fact that the maximum counter E.M.F. set up during electrolysis of water exceeds 3 volts, that the first action of the current is to break up the water (and similarly for other electrolytes) into *free atoms* of the components, which atoms then rearrange themselves into molecules either as a secondary action or under the condensing action of the electrodes, or both; whence it would seem that *the sum of the heat of condensation of hydrogen atoms to molecules together with that of oxygen atoms to molecules exceeds that of the union of the hydrogen and oxygen atoms together to form molecules of liquid water.*

66. The phenomena connected with "subsequent polarization" (§ 58, footnote) are just as readily explained by the theorem as those relating to the counter E.M.F. set up whilst the current is passing. When visible decomposition has taken place for some time, so that the electrodes are surrounded respectively with a saturating aura of hydrogen and of oxygen, varying in density from the highest to the lowest possible value, and when, in addition, there are in the vicinity of the electrodes more or less of the "nascent" products of electrolysis, the counter E.M.F. set up during the passage of the current, and therefore representing the polarization at the instant of rupture, is (§ 61)

$$e=\mathrm{E}_1+[\Sigma\{(1-n)\mathrm{H}\}-\Sigma(nh)-\Sigma(\mathbf{H})]\chi\mathrm{J}.$$

After the lapse of a given interval of time (however small), more or less of the "nascent" products have spontaneously changed into the final products, viz. oxygen and hydrogen, thereby diminishing the value of $\Sigma\{(1-n)\mathrm{H}\}$, and consequently lessening the value of e, the "polarization." Even after sufficient time has elapsed for the whole of the "nascent products" to have thus transformed themselves, the diffusion of water containing dissolved oxygen to the hydrogen electrode from the other, and of water containing dissolved hydrogen from the hydrogen electrode to the other, will still cause, by chemical action, a continual removal of portions of the aura of attracted gas round each electrode, thereby causing a continual increase in the mean value of $\Sigma(nh)$*, and hence a further diminution in the value of e; finally the polarization must become 0, when

$$\{\Sigma(nh)+\Sigma(\mathbf{H})\}\chi\mathrm{J}=\mathrm{E}_1=1{\cdot}50 \text{ volt}.$$

* Gladstone and Tribe have shown (Chem.-Soc. Journ. Trans. 1878, p. 306) that hydrogen condensed by surface-attraction upon or occluded by certain solids (*e. g.* palladium, platinum, or carbon) exerts a much

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Experiments on the rate of diminution of the "subsequent polarization" and the circumstances influencing it are detailed later on (§ 69 *et seqq.*).

67. When an electrolytic cell (*e. g.* a voltameter), the electrodes of which are destitute of attracted films of gas or occluded gases, is connected with an electromotor and a current of very short duration sent through it, if the amount of decomposition produced be so small that the hydrogen- and oxygen-development equivalent to the quantity of electricity passing are only small fractions respectively of the amount requisite to saturate the electrodes, no visible development of gas will ensue; the polarization at the instant of rupture of current will then be (§ 62)

$$e = \mathrm{E}_1 - \Sigma(h)\chi\mathrm{J},$$

the value of h depending on the particular amounts of condensation to which the evolved products are condensed. On passing the current from the same electromotor for the same time again, a slightly less amount of current will flow; for the mean counter E.M.F. set up during this second interval of time will be somewhat greater than that during the first, whilst the total resistance in circuit is the same, whence the current (which $= \frac{\Sigma(\mathrm{E}) - e}{\Sigma(\mathrm{R}) + \mathrm{R}}$, § 57) must lessen. Similarly, on passing the current again and again for equal intervals of time, quantities of electricity will pass in each interval, gradually diminishing in amount; whilst the "subsequent polarization" continually increases, the voltameter acting as a condenser, the capacity of which is simply measured by the power of the electrodes to condense upon their surfaces and occlude internally the products of electrolysis. On discharging a charged condenser by causing it to generate a regular succession of currents of equal but very short duration as to

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more energetic reducing action upon various substances than ordinary free hydrogen, the same result for palladium charged with occluded hydrogen (so-called hydrogenium) having been previously observed by Graham (Proc. Roy. Soc. xvii. p. 212, 1869). This result is not due to the condensed hydrogen being capable of evolving more heat by action on the substances in question than would be produced by the same quantity of free gaseous hydrogen, because the reverse is the case, the difference being the heat given out in condensing the hydrogen from the ordinary gaseous condition to the condensed form obtaining in the particular cases in question. The "catalytic" action of spongy platinum exemplified in Dobereiner's lamp, for instance, also illustrates this fact. The effect of the condensation, therefore, is analogous to that of increased temperature, causing chemical actions to take place that would not ensue (at least at any measurable rate) under other conditions, *i. e.* less condensation or lower temperature respectively.

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time, the quantity of electricity conveyed in each interval must at first be nearly the same, but will gradually diminish. These and various other analogous points in connexion with the behaviour of a voltameter as a condenser have recently been quantitatively studied by Herwig (Wied. *Ann.* ii. p. 601, and vi. p. 305), whose results are perfectly in accordance with the deductions from the general theorem above stated.

68. It is evident that the considerations that apply to the form of voltaic combination produced by the products of decomposition formed by the passage of a current through the decomposing-cell will equally apply if the same products of decomposition are introduced into the cell in other ways; so that if two platinum plates are exposed, the one to water impregnated with ordinary oxygen gas, the other to water containing dissolved hydrogen, the two waters meeting, the attractive action of the plates will cause a more or less nearly saturated aura to be formed round each plate, and a difference of potential will be set up between the plates representing the work that would be gained by the combination of the condensed oxygen and hydrogen: in fact, the form of Grove's gas-battery thus produced will be identical in all respects with that developed when water is decomposed in a voltameter, except that in the latter case the polarization is more or less enhanced by the presence of the "nascent" products of electrolysis, at least until these have become wholly converted into the ultimate products. Such a gas-battery must therefore develop an E.M.F. expressed by the value of the formula

$$e=\mathrm{E}_1+[\Sigma\{(1-n)\mathrm{H}\}-\Sigma(nh)-\Sigma(\mathbf{H})]\chi\mathrm{J}$$

now obtaining; that is, since $n=1$,

$$e=\mathrm{E}_1-\{\Sigma(h)+\Sigma(\mathbf{H})\}\chi\mathrm{J},$$

or, since $\Sigma(\mathbf{H})$ must be extremely small, if not wholly negligible, practically,

$$e=\mathrm{E}_1-\Sigma(h)\chi\mathrm{J}.$$

Hence *the E.M.F. of a hydrogen and oxygen gas-battery cannot exceed* 1·50 *volt*, which experimentally is found to be the case. Any thing that affects the value of $\Sigma(h)\chi\mathrm{J}$ must affect the E.M.F. of a gas-battery; accordingly the E.M.F. is found to vary largely with the temperature, the nature of the plates, the fluid in which the gases are dissolved, the state of concentration of the gas-solutions therein, the presence of other gases, &c.; and in particular it is found to diminish with the strength of the current generated (Morley, *loc. cit. suprà*); for the more rapidly the films or auræ of gases attracted and those occluded are used up by chemical

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action, the less will be the mean densities of the auræ, and hence the greater will be $\Sigma(h)$ and the less the value of e.

Experiments on the Influence of dissolved Gases on the Rate of Fall of the Polarization existent after rupture of the original current (Subsequent Polarization).

69. It results from the general theorem above stated (§ 61), that the rate of fall of the "subsequent polarization" in any given electrolytic cell (*e. g.* a voltameter) must be more rapid when the circumstances are such as to increase the numerical value of the term $\Sigma(\mathbf{H})$ than when the term is negligible; that is, the rate of fall must be a minimum (under any given conditions) when the fluids surrounding the electrodes (themselves unattacked by the products of electrolysis) have no chemical or physical action on these products, and when the electrodes have attracted and occluded as much of the products as they possibly can, but must be greater when these conditions are not fulfilled; so that in any ordinary voltameter, if the fluid surrounding the — electrode contain dissolved oxygen, the rate of fall must be greater than would be the case if no dissolved oxygen were present to act on the hydrogen aura; whilst, *cæteris paribus*, the larger the electrodes within certain limits, the less will probably be the effect of the oxygen dissolved in a limited amount of surrounding fluid on the hydrogen aura as a whole. Experiments on the rate of fall of "subsequent polarization" have been already made by Ayrton and Perry (Journ. Tel. Eng. v. p. 391, 1876), and the results expressed as curves; these observers, however, did not particularly examine the influence of varying amounts of dissolved air &c. on the rate of fall. On the other hand, Helmholtz, Fleming, and others have noticed that when water freed from dissolved air by boiling is electrolysed, the "subsequent polarization" diminishes less rapidly than when water containing dissolved air is employed.

The mode of observation adopted for the purpose of verifying the above deductions from the theorem was much the same as that used by Ayrton and Perry (*loc. cit.*). The electrodes of a given voltameter were connected with an electrometer, a suitable resistance being also in circuit, so that a current of known strength could be passed for any required length of time: to measure the current passing it was found convenient to employ a method based on Ohm's law, viz. reading off the difference of potential set up by the current between the ends of a known resistance by means of a quadrant-electrometer. This method, first, is independent of the errors (due to possible change of zero, horizontal magnetic component, &c.) of ordinary galvanometers, which cannot conveniently be verified

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without throwing them out of circuit or otherwise varying the current passing by altering the total resistance in circuit, which is impracticable in experiments in which it is essential that the current should not sensibly vary during long periods of time,—and, secondly, enables the value of an extremely minute current to be determined with as much accuracy as that of a much stronger one, and even with greater accuracy; for whilst the current from a given electrometer is diminished by throwing more and more resistance into circuit, the difference of potential between the ends of the resistance thus thrown into circuit is not only not decreased but is even increased. For if e_1 be the E.M.F. of the electrometer, R_1 the resistance the difference of potential between the ends of which e_2 is measured, and R_2 the resistance of the rest of the circuit, the current passing

$$C = \frac{e_2}{R_1} = \frac{e_1}{R_1 + R_2},$$

whence

$$e_1 - e_2 = CR_2.$$

Hence, as C diminishes e_2 increases, and consequently a larger electrometer-deflection is obtained with a smaller current. Tolerably strong currents develop appreciable amounts of heat in the resistance-wire, thus increasing its resistance and introducing an error in deficiency in the current; so that the method is more particularly applicable for minute currents.

70. The difference of potential set up between the electrodes of a voltameter by the passage of a current C is $E = e + CR$, where e is the counter E.M.F. set up, and R the resistance of the voltameter (§ 57). If R do not exceed 100 ohms (100×10^9 C.G.S. units of resistance), and C do not exceed 0·0001 weber (0·00001 C.G.S. current-unit), the value of C R does not exceed $100 \times 10^9 \times 0{\cdot}00001 = 0{\cdot}01 \times 10^8 = 0{\cdot}01$ volt; whilst, whatever the value of C and R, the E.M.F. equal to the product CR is readily calculable. If now the difference of potential between the electrodes E be read off (in terms of the scale of the quadrant-electrometer) whilst the current is passing, by noticing the deflection of the spot of light from the zero (readings being taken on one side only), and the current be then interrupted, a motion backwards of the spot through a scale-length equivalent to CR will instantly ensue, and subsequently a continuous motion backwards as the value of e falls; if, however, CR be equivalent to considerably less than half a scale-division of the electrometer, the first sudden motion backwards will be entirely inappreciable. In the following experiments the electrometer was so arranged that 1 scale-division represented about 0·02 volt (readings being taken on one side only of the

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zero); and consequently as long as CR was less than 0·01 volt (*i. e.* as long as R did not exceed 100 ohms and C 0·0001 weber) the first sudden motion was less than 0·5 scale-division and was consequently practically inappreciable.

(I.) Surface of each platinum electrode 3·2 square centims. Value of R=7·6 ohms; C=·000005 to ·0001 weber; so that CR was quite negligible; value of E at moment of rupture of current = 1·60 volt (average). The following numbers were obtained as the average of a number of concordant observations; the differences between the values obtained on repetition of an experiment were, as might be expected, much greater in the experiments of the A class than in the others.

Fall of E in volts.

Time in seconds since rupture of current.	A. Unboiled acid.	B. Acid well boiled and used as soon as cold.	C. Voltameter charged with acid saturated with hydrogen and oxygen at the two electrodes respectively.
5	·25	·20	·09
10	·33	·25	·13
15	·39	·32	·15
20	·43	·37	·17
30	·48	·45	·20
60	·59	·58	·26
120	·70	·70	·33
180	·75	·75	·35

(II.) Surface of each electrode 8·0 square centims. Value of R = 11·2 ohms, current ·000005 to ·0001 weber; so that CR was practically negligible. Average value of E at moment of rupture of current 1·80 volt.

Fall of E in volts.

Time in seconds.	A.	B.	C.
5	·26	·22	·19
10	·33	·27	·23
15	·39	·30	·25
20	·43	·32	·26
30	·48	·36	·28
60	·52	·38	·29
120	·56	·40	·295
180	·58	·41	·30

In precisely the same way analogous numbers were obtained in many other series of experiments, the details of which it is unnecessary to quote. Whether the electrodes were of gold,

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platinum, or carbon, whatever their size, and whatever the value of E, in all cases the general character of the numbers was the same as in these two examples; i. e. *the rate of fall with acid containing dissolved air was greater than that with acid freed from the greater part of the air dissolved by boiling; whilst this again was greater than that with acid from which the last traces of dissolved air had been removed* (from the fluid surrounding the hydrogen-pole) by the slow evolution of hydrogen therefrom, so as to saturate the fluid with hydrogen and remove the dissolved oxygen by "electrolytic convection." As the removal of the last traces of oxygen became more and more nearly effected, the rate of fall of polarization (for a given initial value) *gradually approached a minimum value*, beyond which it never sank, this minimum doubtless representing the rate of alteration produced in the auræ by diffusion from one electrode to the other.

71. The following experiments are specimens of a number of other analogous ones, all of which yielded the same general result, viz. that, other things being equal, *the rate of fall of polarization is less the larger the electrode surface.* The voltameters were precisely like those used in the above experiments, consisting of U-tubes with pieces of platinum-foil bent into cylinders and attached to platinum wires fused into glass tubes which served as mercury-cups—access of external air being prevented by closing each of the two open ends of the U-tubes with a doubly perforated cork (of india rubber, or paraffin), the mercury-cup tube passing air-tight through one perforation and a delivery-tube (bent over and dipping under mercury or oil) through the other, so that evolved gas could escape without admission of air. The voltameters, being filled with boiled acid containing about 27 grammes of H_2SO_4 per 100 cubic centims., were then connected with a pair of Minotti cells, a large resistance being in circuit; so that a feeble current passed, evolving one or two cubic centims. of hydrogen per day. After some days (generally a week to a fortnight) the minimum rate of fall of polarization was arrived at; when this was the case, the electrodes were kept for another day at some particular difference of potential, arrived at by suitably altering the current passing and experimented with throughout, and a series of observations made; the electrodes were then again brought to the same difference of potential by passing the current again for some time, and another series of observations made; and so on. Finally, the following values were obtained from several such series of concordant observations, the experiments being alike in every respect save size of electrodes—the strength of the acid and the distance apart of the nearest portions of the electrodes being as nearly as possible the same

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throughout, and the U-tube being the same; so that diffusion must have gone on at as nearly as possible the same rate throughout. The numbers in the column headed "largest" were obtained with electrodes each exposing 27·5 square centims. surface; those in the columns headed "medium" and "smallest" with electrodes exposing respectively 3·2 and 0·2 square centims. of surface. In every case the value of CR was practically inappreciable.

(I.) Value of E on breaking circuit = 1·60 volt.

Fall of E in volts.

Time, in seconds.	Largest.	Medium.	Smallest.
5	·05	·09	·19
10	·08	·13	·27
20	·10	·17	·37
30	·12	·20	·46
60	·15	·26	·61
120	·19	·33	·75
180	·22	·35	·84

(II.) Value of E on breaking circuit = 1·05 volt.

5	less than ·01	·03	·17
10	·01	·04	·25
20	·02	·05	·31
30	·03	·06	·37
60	·04	·07	·44
120	·06	·09	·49
180	·07	·10	·52

It is evident from these numbers, specimens of many similar experiments, that the predictions from the general theorem mentioned in § 69 are completely verified by experiment. It may be noticed in passing that with the "largest" voltameter, when the value of E on breaking circuit was below 1 volt, the minimum rate of fall of polarization was so slow that no visible motion of the spot of light occurred even during many seconds, and with still lower values of E even after several minutes.

72. The following experiments are also of interest in connexion with this subject, as indicating how the rate of fall of polarization is affected not only by the removal of the auræ of gas round the electrodes by diffusion from one to the other of fluid containing dissolved gas, but also by the solution of the aura in the fluid itself (if not saturated with the gas constituting the aura), and also by the passage of gas into or out from the interior of the electrode, thus causing a diminution in the aura or *vice versâ*. Platinum- or gold-foil electrodes of various sizes being arranged in U-shaped voltameters like those just described, a gentle current was passed for some days, so as

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to saturate as far as possible, not only the liquids surrounding the electrodes with the gases respectively evolved on the electrodes, but also the interior of the foils with occluded gases. The electrodes were then rapidly removed, rinsed with recently boiled just cool acid to remove adherent solution of hydrogen or oxygen, and dipped into a U-tube containing the same recently boiled just cool acid. On connecting the two pieces of foil with a quadrant-electrometer a considerable potential difference was at once indicated. The two pieces of foil were then connected by a shortcircuiting wire for a short time (ten seconds usually); on removal of the wire, the foils being still connected with the electrometer, the spot of light moved regularly for a considerable time, indicating a gradual increase in the E.M.F. of the voltaic arrangement thus produced, from 0 at the moment of removal of the shortcircuiting wire up to a maximum, which was maintained for some time, after which the E.M.F. gradually fell, precisely as in the experiments just described. It is evident that this development and rise of the E.M.F. set up was due to the formation and increase of an aura of gas round each electrode, the source of which was clearly the gas occluded internally in each respectively, the which gas now passed outwards by a reversal of the process by which it formerly passed inwards. The following three sets of readings (averages of fairly concordant repetitions of each experiment) will serve as specimens of a large number of analogous results; the numbers show the E.M.F. set up after the annexed time had elapsed:—

Voltameter used	"Largest."	"Largest."	"Medium."
Value of E on breaking circuit in the first instance (had been maintained for several days previously)	1·05 volt.	1·95 volt.	2·20 volts.
Time, after removal of shortcircuiting wire:—	volt.	volt.	volt.
5 seconds.....	·14	·35	·39
10 „	·19	·41	·44
15 „	·21	·43	·47
20 „	·23	·45	·50
30 „	·25	·47	·52
1 minute	·27	·52	·55
2 minutes	·28	·55	·58
3 „	·29	·57	·59
7 „	·29	·59	·60
10 „	·28	·59	·58
15 „	·28	·57	·55
30 „	·27	·53	·50
1 hour.....	·26	·49	·34
2 hours	·245	·44	·31
3 „	·235	·42	·29
4 „	·23	·40	·28

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In these three cases the maximum E.M.F. was attained respectively after about 3–7, 7–10, and 7 minutes. The value of the maximum was much higher in the second case than in the first, indicating a much greater amount of gas occluded, as might be anticipated, since a much more rapid current flowed during the period before the electrodes were disconnected from the battery, the current being near to ·000001 and ·000020 weber during these periods in the first and second cases respectively. Notwithstanding, however, that a still more rapid current (about ·000025) flowed in the third case, the maximum attained was scarcely higher than that in the second, because the smaller surface of the electrode caused the rate of loss of aura by solution in the unsaturated surrounding fluid and diffusion away of the solution thus formed to another part of the U-tube to be relatively larger in this case; consequently the rate of fall after the maximum had been reached was considerably more rapid.

73. A curious effect is sometimes produced by the gradual passage outwards from electrodes of occluded gases and the formation of an aura round each, thus virtually setting up a kind of Grove's gas battery. If the current passing through a voltameter be suddenly largely diminished (but not altogether interrupted) by diminishing the E.M.F. of the battery used, a considerable decrease is brought about in the counter E.M.F. set up in the voltameter; that is, the value of $e=E_1+[\Sigma\{(1-n)H\}-\Sigma(nh)-\Sigma(H)]\chi J$ diminishes. The passage outwards of occluded gas from the interior of the electrodes, however, tends to increase the aura round each electrode, and consequently to diminish $\Sigma(nh)$, and hence to increase e; and the result is that the voltameter behaves for a while like a more powerful opposed battery; so that *the current for a time passes in the opposite direction to that due to the primary electromotor*: of course this effect can only be produced when the E.M.F. of the primary electromotor (after the reduction in its E.M.F.) does not exceed some particular limit. If the experiment be modified by shortcircuiting the electrodes of the voltameter before again coupling them to the battery after reducing its E.M.F., the current will at first pass in the normal direction due to the battery; but as the occluded gases pass outwards from the interior of the electrodes and so form an aura round each, the current passing gradually diminishes in strength until it becomes *nil*, and finally passes in the opposite direction, just as it would have done at first had the voltameter not been shortcircuited. For example, a current of about ·00005 weber (capable of evolving about 0·5 cubic centim. of hydrogen per day) was passed for several days through

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a voltameter till the liquids surrounding the electrodes were saturated with oxygen and hydrogen respectively, for which purpose a battery of two Minotti cells was used. One Minotti cell was then excluded from the circuit, a resistance of 50,000 ohms being included therein (the current being measured by determining the difference of potential set up between the ends of this resistance, as described in § 69), and the voltameter-plates shortcircuited for a few seconds. After removing the shortcircuiting wire, the current passed at a rapidly slackening rate in the direction due to the Minotti cell; but in a very few minutes it ceased to flow altogether, and then began to flow in the opposite direction. The shortcircuiting of the voltameter-plates was then repeated for two minutes, after which the current flowed in the normal direction at a gradually slackening rate: after one hour the current still flowed in the normal direction and had a value of +0·0000034 weber; but soon it became reduced to zero again, and then flowed in the reverse direction, having a value of −0·0000017 weber after two hours had elapsed since the second shortcircuiting. After twenty hours more the current was so small that its direction was inappreciable; but on removing the electrodes and igniting them and then replacing them, it flowed continuously in the normal direction. Similar results were obtained in many other analogous experiments.

The gradual setting-up again of an E.M.F. after shortcircuiting the electrodes of a voltameter has been already previously noticed by Ayrton and Perry, and the effect generally compared by them to the residual charge of a Leyden jar; they do not, however, appear to have observed the gradual extinction and reversal of the current just described.

74. It should further result from the general theory of electrolysis above stated, that if the electrodes of a voltameter are shortcircuited whilst still in contact with the fluid saturated with gases surrounding them, on removing the shortcircuiting wire the reproduction of an aura round each electrode will take place much more rapidly than will be the case were the electrodes removed and placed in fresh acid not saturated with gas, as in the experiments described in § 72; for in the former case the loss of aura produced by the passage outwards of occluded gas owing to solution in the liquid will be much smaller than that taking place in the latter case. That this is so the following numbers show, samples of numerous similar observations made, each series of numbers being the average of several fairly accordant sets of determinations.

(I.) Platinum electrodes, exposing a surface of 27·4 square centims. each, kept at a difference of potential of 1·95 volt in

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each case for several days before the observations were made, the current passing being near to ·00002 weber. Shortcircuited for ten seconds.

Period since cessation of the shortcircuiting.	Electrodes not removed from the acid used whilst the original current passed.		Electrodes removed, rinsed, and placed in fresh acid (recently boiled and just cooled) before shortcircuiting.	
		volt.		volt.
During 1st minute .	Steady rise to	·86	Steady rise to	·52
2nd ,,	Further rise to	·95	Further rise to	·55
3rd ,,	,, ,, ,,	1·03	,, ,, ,,	·56
3rd–5th ,,	,, ,, ,,	1·11	,, ,, ,,	·58
5th–10th ,,	,, ,, ,,	1·24	Attained maximum	·59
10th–20th ,,	,, ,, ,,	1·26	Slight fall to	·55
20th–30th ,,	Attained maximum	1·27	Further fall to	·53
30th–60th ,,	Constant at	1·27	,, ,, ,,	·49
60th–120th ,,	Slight fall to	1·265	,, ,, ,,	·44
120th–180th ,,	,, ,, ,,	1·26	,, ,, ,,	·42
180th–240th ,,	,, ,, ,,	1·25	,, ,, ,,	·40

(II.) Electrodes exposing a surface of 3·2 square centims. each, kept at potential-difference of 2·20 volts, the current being near ·000025 weber. Shortcircuited for ten seconds.

Period since cessation of the shortcircuiting.	Electrodes not removed from the acid used whilst the original current passed.		Electrodes removed, rinsed, and placed in fresh acid (recently boiled and just cooled) before shortcircuiting.	
		volt.		volt.
During first 7 minutes .	Steady rise to	·60	Steady rise to	·60
7th–20th minute.	,, ,, ,,	·63	Slight fall to	·54
20th–30th ,,	Constant at	·63	Further fall to	·50
30th–60th ,,	Slight fall to	·61	,, ,, ,,	·34
60th–120th ,,	Further fall to	·57	,, ,, ,,	·31
120th–180th ,,	,, ,,	·51	,, ,, ,,	·29
180th–240th ,,	,, ,,	·49	,, ,, ,,	·28

75. The absolute mass of gas occluded by even moderately large foil electrodes is but minute ; on Sprengelpumping at a red heat a platinum-foil electrode of about 160 square centims. total surface (about 16 centims. long, and 5 broad), weighing four grammes, only barely recognizable traces of hydrogen were obtained, even when the electrode had been used for a long time and hydrogen copiously evolved from its surface during electrolysis. The presence of occluded hydrogen, however, can readily be observed by chemical tests

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such as the reduction of ferric to ferrous salts, &c. One of the most sensitive was found to be that used by Osann (Pogg. *Ann.* xcv. pp. 311 and 315), viz. washing the electrode (in recently boiled just cool distilled water), and then immersing it in a clear brown-yellow solution of mixed ferric chloride and potassium ferricyanide. In a few minutes, according to the quantity of hydrogen occluded, a distinct blue precipitate forms on the surface of the platinum, due either to the conversion of ferri- into ferrocyanide, or to the reduction of ferric to ferrous chloride, or both together. In this way the presence of hydrogen can be distinguished in a platinum electrode (ignited in oxygen previously to use in the voltameter), even when no visible evolution of hydrogen gas from its surface has occurred, the E.M.F. of the battery used being too small to admit of a steady current passing at a rate more rapid than that representing the rate at which removal of the aura of condensed gas by the effects of diffusion (§ 70) takes place—for instance, when the battery-E.M.F. is not greater than 1·4 to 1·5 volt (Part iv. § 84 *et seq.*). In applying this test, however, too much reliance must not be placed on the formation of a blue precipitate after the lapse of a long time; for organic matters and reducing gases and vapours from the air are apt to be absorbed by the ferric ferrocyanide liquor, causing reduction, especially under the influence of light. Moreover even platinum recently ignited and cooled in oxygen, when placed in the solution, becomes covered with a thin blue film after a considerable time (some hours or days), suggesting the possible reduction of ferricyanide to ferrocyanide with simultaneous production of platinocyanide. Gold acts in this way much more rapidly than platinum. In applying this test in the above-cited observations, check experiments were always made with a second piece of platinum foil of about the same size recently ignited: the blue deposit on the foil used as electrode was then found to be notable or considerable when none at all was visible on the check piece.

76. From the general theory of electrolysis and the experiments above described, and the much more numerous analogous observations made but not described in detail for the sake of saving space, it results that the condition of the aura round each electrode of a voltameter that has been used for decomposing water at a given period since rupturing the original current is influenced by many circumstances. The aura round the — electrode, for example, has a particular mean density at the moment of breaking circuit, whilst that round the + electrode has some other mean density, the

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polarization then existent being governed by the proportions of "nascent" hydrogen and oxygen that were being evolved the instant previously and the mean densities of the auræ, *i. e.* by the values of n_1 and n_2 and h_1 and h_2 in the expression

$$e=\mathrm{E}_1+\{(1-n_1)\mathrm{H}_1-n_1h_1+(1-n_2)\mathrm{H}_2-n_2h_2-\Sigma(\mathbf{H})\}\chi\mathrm{J}$$

(§ 61). After the lapse of a given time, however, more or less of the "nascent" hydrogen and oxygen have rearranged themselves, and more or less of the original aura round each electrode has become reconverted into water by the effects of diffusion (bringing oxygen-containing fluid to the hydrogen-electrode and *vice versâ*); so that from these causes n_1 and n_2 have increased, each of these changes diminishing the value of e, the polarization. Simultaneously, however, each aura has been more or less reinforced by the passage outwards of occluded gas from within, each of these changes tending to increase e. Presumably the greater the extent to which the aura was wasted, so to speak, by diffusion, the more rapidly would this outward passage of occluded gases take place; so that finally the rate of fall of polarization is gradually retarded, first because as the "nascent" hydrogen and oxygen become expended by spontaneous rearrangement this source of fall of polarization regularly diminishes, and secondly because the greater the fall the more energetic is the restoring influence of the outward passage of the occluded gases, at least until the amount occluded becomes materially reduced in quantity. When the amount of fall is such that the rate of outward passage of occluded gases almost balances the rate of wasting of aura through diffusion, the rate of fall of polarization becomes almost insensible, at any rate for periods of time not too prolonged. This point was reached in many of the experiments, in particular those with the "largest" voltameter and polarizations of values not exceeding a few tenths of a volt (§ 71). Ultimately, however, the occluded gases become exhausted and the auræ diminish to inappreciable quantities, the value of e diminishing to a quantity too small to measure. This result requires an enormous length of time to bring it about by the action of diffusion only; if, however, the voltameter-plates are united by an external connecting-wire, a current flows through that wire, and work is done at the expense of the energy stored up, so to speak, in the auræ and occluded gases of the electrodes. The capacity of the condenser virtually formed by the voltameter-plates simply means the quantity of electricity that can be made to flow in this way, and will approach the more nearly to the quantity of electricity that passed in forming the auræ and charging the

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electrodes with occluded gases the less has been the waste of auræ by diffusion &c. During the passage of the current, the auræ and the occluded gases are used up by chemical action, just as the zinc is in an ordinary cell; and as their quantity diminishes, the effective E.M.F. of the voltaic couple formed by the plates also diminishes.

If through leakage of the apparatus, or other causes, atmospheric air obtains access to the liquid surrounding the —electrode, the dissolved oxygen thence resulting will of course tend to reduce the aura of hydrogen by a chemical action of a kind akin to the "local action" of an ordinary voltaic element, *i. e.* not contributing to the work done by the current generated. In this case the rate of fall of the "subsequent polarization" (the electrode not being connected by a wire so as to produce a current) will be more or less enhanced above the rate due to diffusion only, as above described (§ 70); whilst if the electrodes are connected and a current is made to pass, the E.M.F. of the voltaic couple thus produced will diminish more rapidly than would be the case were no dissolved oxygen present in the liquid round the hydrogen-plate, just as the zinc of an ordinary cell wastes more rapidly when there is local action than it does when there is not, other things being equal.

XXV. *On Copying Diffraction-gratings, and on some Phenomena connnected therewith.* By Lord Rayleigh, F.R.S., *Professor of Experimental Physics in the University of Cambridge.**

IN the Phil. Mag. for February and March 1874 I gave an account of experiments in the photographic reproduction of gratings ruled with lines at a rate of 3000 and 6000 to the inch. Since that time I have had further experience, extending to more closely ruled gratings, and have examined more minutely certain points which I was then obliged to leave unexplained. The present communication is thus to be regarded as supplementary to the former.

Some years ago Prof. Quincke described an unphotographic process by which he had succeeded in copying engraved glass gratings. He began by depositing a thin coating of silver by the chemical method upon the face of the grating. The conducting layer thus obtained was then transferred to an electrolytic cell, and thickened by the deposit of copper, until stout enough to be detached from the glass substratum. In this way he prepared an accurate cast of the

* Communicated by the Author.

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glass surface, faced with highly reflecting silver. Since the optical depth of the lines is increased some four times, these gratings usually give much brighter spectra than the glass originals.

Prof. Quincke was kind enough to send me some specimens of his work, giving extremely beautiful spectra. I found, however, that, though carefully preserved, these gratings deteriorated after a time, apparently either from insufficient thickness, or from imperfect adhesion of the silver layer. In my own attempts I endeavoured to remedy this defect by not allowing the silver to dry before transference to the electrolytic cell, and by commencing the electric deposit with a *silver* instead of with a *copper* solution. I did not, however, succeed in finding a thoroughly satisfactory plating-liquid. In the ordinary cyanide solution the silver was at once loosened from the glass. In other solutions the grating could be immersed with impunity, but the film began to strip as soon as the current passed. Using *acetate* of silver, however, I was able to obtain a certain degree of thickening. I also found advantage from commencing the deposit of copper with a *neutral* solution. After the layer had attained a moderate thickness, its edges were dipped in melted paraffine; and it was then transferred to the usual acid solution of copper. I did not find it necessary to take any precautions against too great an adhesion between the silver and the glass.

These copies are now four years old, and they do not seem to have deteriorated. A slight yellow tarnish, due probably to sulphur, can be removed with cyanide of potassium. There is, however, one defect which I have not been able to avoid. The silver surface is never sufficiently flat to bear much magnifying-power. Unless this difficulty can be overcome, the use of such gratings must be limited to cases where brilliancy and not high defining-power is the desirable quality. For most purposes the photographic method of reproduction is to be preferred as far easier and quicker. Among various processes of this kind, I am still inclined to give the preference to that in which collodio-chloride of silver is employed, with subsequent treatment with mercury. The only trouble that I have met with is the tendency of the soluble salts to crystallize in the film; but this can generally be avoided with a little judgment. As these photographs cannot well be varnished, some doubts might have been entertained as to their permanence; but I find that copies now more than seven years old are none the worse. For gratings to be subjected to rough treatment, the various albumen processes offer decided advantages.

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In my former paper I stated my opinion that the photographic method of reproduction would be applicable to lines finer than any that I had then tried (6000 to inch). In the summer of 1879 an opportunity afforded itself of submitting the matter to the test of actual trial through the kindness of Mr. Rutherfurd, who presented me with a beautiful glass grating containing nearly 12,000 lines, ruled at the rate of 17,280 to the inch. The copies, taken with suitable precautions to secure a good contact, were completely successful, so far as the spectrum of the first order is concerned. Indeed careful comparison showed no appreciable difference between the defining-power of the original and of the copies; and with respect to brightness some of the copies had the advantage. On a former occasion * I have shown that the theoretical resolving-power in the orange region of the spectrum is equal to that obtainable from a prismatic spectroscope with $12\frac{1}{2}$ per cent. of "extra dense flint;" and I have no reason to think that the actual resolving-power fell far short. This is a considerable result to obtain with a photograph which may be taken in half an hour at a cost of two or three shillings.

The case is different, however, when we turn to the spectrum of the second order. Used in this way the original gives magnificent results; but they are not reproduced in the copies. Some parts of the photograph will sometimes show a faint spectrum of the second order; but it is usually traversed by one or more dark bands, whose nature will presently be examined more at length.

As a rule, glass (or at any rate transparent) originals only would be used for purposes of reproduction; but as a matter of curiosity I tried what could be done in copying an original ruled on speculum-metal. The specimen experimented upon was similar to my own, both as to the total number of lines and as to the degree of closeness, and belongs to Mr. Spottiswoode, to whom I am indebted for the loan of it. In this case the light of the sun had to pass through the sensitive film before it could reach the speculum-metal; it was then reflected back, and in *returning* through the film impressed the ruled structure. No very brilliant result was to be expected; but I succeeded so far as to obtain a copy which gave very fair results when tested upon the sun.

In my former paper I mentioned that when a spectrum of high order is thrown upon the eye, there usually appear upon the grating a certain number of irregular dark bands. These are the places at which the copy fails to produce the spectrum in question. With lines not closer than 3000 or 6000

* Phil. Mag. Oct. 1879.

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to the inch, and with reasonably flat glass as support to the photographic film, these bands rarely invade the first or second spectrum. When, however, we come to 17,000 lines to the inch, it requires pretty flat glass and some precautions in printing to keep even the first spectrum free from them.

It was obvious from the first that the formation of these bands was a question of the distance between the ruled surface of the original and the sensitive film; but it is only within the last year or so that I have submitted the point to special experiment. For this purpose I substitute for plane-parallel glass as a substratum for the sensitive film the convex surface of a lens of moderate curvature. As in the experiment of Newton's rings, we obtain in this way an interval gradually increasing from the point of contact outwards, and thus upon one plate secure a record of the effect upon the copy of varying degrees of closeness. When a spectrum of any order is thrown upon the eye, those places upon the grating where the spectrum in question fails appear as dark rings. My first experiment of this kind was made with the Rutherfurd grating, in order principally to find out how close a contact was really necessary for copying. From the diameter of the first dark ring, in conjunction with a rough estimate of the curvature of the lens, I concluded that the interval between the surfaces should nowhere much exceed $\frac{1}{10000}$ of an inch. It appeared at the same time that the chance was remote of obtaining a satisfactory performance in the spectrum of the second order. About this time the theoretical views occurred to me which will presently be explained, and I purposed to check them by more careful measurements than I had yet attempted. In the course of last summer, however, I found accidentally that Fox Talbot had made, many years ago*, some kindred observations; and the perusal of his account of them induced me to alter somewhat my proposed line of attack. It will be convenient to quote here Fox Talbot's brief statement:—

"About ten or twenty feet from the radiant point, I placed in the path of the ray an equidistant grating† made by Fraunhofer, with its lines vertical. I then viewed the light which had passed through this grating with a lens of considerable magnifying-power. The appearance was very curious, being a regular alternation of numerous lines of bands of red and green colour, having their directions parallel to the lines of the grating. On removing the lens a little further from the grating, the bands gradually changed their colours,

* Phil. Mag. Dec. 1836.

† A plate of glass covered with gold leaf, on which several hundred parallel lines are cut, in order to transmit the light at equal intervals.

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and became alternately blue and yellow. When the lens was a little more removed, the bands again became red and green. And this change continued to take place for an indefinite number of times, as the distance between the lens and grating increased. In all cases the bands exhibited two complementary colours.

"It was very curious to observe that, though the grating was greatly out of the focus of the lens, yet the appearance of the bands was perfectly distinct and well defined.

"This, however, only happens when the radiant point has a *very small* apparent diameter, in which case the distance of the lens may be increased, even up to one or two feet from the grating, without much impairing the beauty and distinctness of the coloured bands. So that if the source of light were a mere mathematical point, it appears possible that this distance might be increased without limit; or that the disturbance in the luminiferous undulations caused by the interposition of the grating continues indefinitely, and has no tendency to subside of itself."

It is scarcely necessary to point out that what was seen by the eye in this experiment in any position of the magnifying lens was the same as would have been depicted upon a photographic plate situated at its focus, at least if the same kind of rays had been operative in both cases. Talbot's observations are therefore to the point as determining the effect of varying intervals in photographic copying.

On the whole the above description agrees well with what I had expected from theory. It is indeed impossible to admit that the red and green coloration could disappear and revive an *indefinite* number of times. The appearance of colour at all shows that the phenomenon varies with the wave-length, and accordingly that it would (as in all such cases when white light is used) ultimately be lost. Besides the limit imposed by the apparent magnitude of the source of light, there must be another depending upon the variation of wave-length within the range concerned.

In trying to repeat Talbot's experiment I found that even the 3000-to-the-inch grating was too fine to be conveniently employed; and eventually I fell back upon a very coarse grating made some years ago by photographing (with the camera and lens) a piece of striped stuff. By comparison of coincidences with the divisions of fine ivory scale (vernier fashion), the period was determined as ·0104 inch. As a source of light I used a slit placed parallel to the lines of the grating, and backed by a fish-tail gas-flame seen edgeways. In order to observe the appearances behind the grating, a lens of moderate

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magnifying-power was sufficient. This lens was moved gradually back until something distinctive was seen ; and the distance between the lens and the grating was then measured and recorded. In order to render the light more nearly monochromatic, pieces of red or green glass were usually held in front of the eye.

With red light the nearly equal bright and dark bars are seen in focus when the distance of the lens from the grating is $1\frac{3}{4}$ inch. As the distance is increased, the definition deteriorates, and is worst at a distance of $3\frac{5}{8}$. In this position the proper period (·0104 inch) is lost, but subordinate fluctuations of brightness in shorter periods prevent the formation of a thoroughly flat field of view. As the distance is further increased, the definition appears to improve, until at distances $5\frac{3}{4}$ and $6\frac{3}{4}$ it is nearly as good as at first. The definition in an intermediate position such as $6\frac{1}{4}$ is distinctly inferior, but is far from being lost as in position $3\frac{5}{8}$. From the theoretical point of view, to be presently explained, these two positions of extra good definition are not to be distinguished. They relate rather to the sharpness of the *edge* of the band, than to any special prominence of the proper period. At a distance of $7\frac{1}{2}$ we have again a place of worst definition, at $10\frac{1}{4}$ a revival, and so on. These alternations could be traced to a distance of *nine* feet behind the grating.

The accompanying table gives the positions of best and worst definitions for red and green light respectively. Of these the places of *worst* definition could be observed with the greater accuracy ; but none of the observations have any pretensions to precision. The star indicates the position for focus.

Red light.		Green light.	
Best.	Worst.	Best.	Worst.
*$1\frac{3}{4}$	$3\frac{5}{8}$	*$1\frac{1}{4}$	$3\frac{3}{4}$
$5\frac{3}{4}$, $6\frac{3}{4}$	$7\frac{1}{2}$	$6\frac{1}{4}$	$8\frac{5}{8}$
$10\frac{1}{4}$	$12\frac{1}{2}$	11	$14\frac{1}{2}$
$15\frac{1}{4}$	17	$16\frac{1}{2}$	$19\frac{3}{4}$
$19\frac{3}{4}$	$21\frac{7}{8}$	$22\frac{1}{8}$	$25\frac{1}{4}$
$24\frac{3}{4}$	27	$27\frac{3}{4}$	$31\frac{1}{2}$
$29\frac{1}{4}$	$32\frac{1}{8}$	$33\frac{1}{4}$	37
$34\frac{1}{4}$	$37\frac{1}{4}$		

It is evident that the positions for red light gradually fall quite away from the corresponding positions for green light.

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At 19¾, for example, if we use a green glass, we lose sight of the proper period, and have before us an almost uniform field; but if without making any other change we substitute a red glass for the green one, we see the bands again with great distinctness. At about the greatest distance included in the table the positions of best definition are again in coincidence; but here there is an important remark to be made. If, using the green glass, we adjust a needle-point to the centre of a bright band, we find, on substituting the red glass, that the needle-point is now in the centre, not of a *bright*, but of a *dark* band. The fact is that at every revival of definition the image changes sign, in the photographic sense, from positive to negative, or from negative to positive—a clear proof that the appearance in question is not a mere *shadow* in any ordinary sense of the term.

With respect to the numerical values of the distances given in the table, theory indicates that the interval from worst to worst or from best to best definition should be a third proportional to the period of the grating d, and the wave-length of the light λ, *i. e.* should be equal to d^2/λ. In the case of red light, the mean interval from worst to worst is 4·8 inches, and from best to best 4·7. The corresponding numbers for green light are 5·5 and 5·3. In the subsequent calculation, I have used the first stated intervals as probably the more correct.

For the grating employed the actual value of d was ·0104 inch; but a small correction is required for the want of parallelism of the light. The distance of the source was about 27 feet; so that, as the mean distance behind the grating at which the appearances were observed was 1½ foot, the above value of d must be increased in the ratio of 28½ to 27. Thus for the effective d in centimetres, we get

$$2{\cdot}54 \times \frac{57}{54} \times {\cdot}0104.$$

Calculating from this and from the observed intervals a by means of the formula $\lambda = d^2/a$, we get in centimetres

$$\lambda_{(\text{red})} = 6{\cdot}40 \times 10^{-5}, \quad \lambda_{(\text{green})} = 5{\cdot}59 \times 10^{-5}.$$

Direct determination of the mean wave-lengths of the lights transmitted by the red and green glasses respectively gave

$$\lambda_{(\text{red})} = 6{\cdot}64 \times 10^{-5}, \quad \lambda_{(\text{green})} = 5{\cdot}76 \times 10^{-5}.$$

The true wave-lengths are certainly somewhat greater than those calculated from Talbot's phenomenon; but the difference is perhaps hardly outside the limits of experimental error. If

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the measurements were ever repeated, it would be advisable to use a collimating lens as well as a more accurate grating.

The problem of determining the illumination at various points behind a grating exposed to a parallel beam of homogeneous light, could probably be attacked with success by the usual methods of physical optics, if it were assumed that the grating presented uniform intervals alternately transparent and opaque. Actual gratings, however, do not answer to this description, and, indeed, vary greatly in character. I have therefore preferred to follow the comparatively simple method, explained in my book on Sound, §§ 268, 301, which is adequate to the determination of the leading features of the phenomenon.

Taking the axis of z normal to the grating, and parallel to the original direction of the light, and the axis of x perpendicular to the lines of the grating, we require a general expression for the vibration of given frequency which is periodic with respect to x in the distance d. Denoting the velocity of propagation of ordinary plane waves by a, and writing $\kappa = 2\pi/\lambda$, we may take as this expression

$$\begin{aligned} &A_0 \cos(\kappa a t - \kappa z) + A_1 \cos\left(\frac{2\pi x}{d} + e_1\right) \cos(\kappa a t - \mu_1 z) \\ &+ B_1 \cos\left(\frac{2\pi x}{d} + e_1'\right) \sin(\kappa a t - \mu_1 z) \\ &+ A_2 \cos\left(\frac{4\pi x}{d} + e_2\right) \cos(\kappa a t - \mu_2 z) \\ &+ B_2 \cos\left(\frac{4\pi x}{d} + e_2'\right) \sin(\kappa a t - \mu_2 z) + \ldots\ldots, \end{aligned}$$

where

$$\mu_1^2 = \kappa^2 - \frac{4\pi^2}{d^2}, \quad \mu_2^2 = \kappa^2 - \frac{4 \cdot 4\pi^2}{d^2}, \quad \mu_3^2 = \kappa^2 - \frac{9 \cdot 4\pi^2}{d^2}, \text{ \&c.}$$

The series is to be continued as long as μ^2 is positive, *i. e.* as long as the period of the component fluctuations parallel to x is greater than λ. Features in the wave-form whose period is less than λ cannot be propagated in this way, but are rapidly extinguished.

The intensity of vibration, measured by the square of the amplitude, is

$$\begin{aligned} &[A_0 + A_1 \cos\left(\frac{2\pi x}{d} + e_1\right) \cos(\kappa z - \mu_1 z) \\ &+ B_1 \cos\left(\frac{2\pi x}{d} + e_1'\right) \sin(\kappa z - \mu_1 z) \end{aligned}$$

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$$+A_2\cos\left(\frac{4\pi x}{d}+e_2\right)\cos(\kappa z-\mu_2 z)+\ldots\ldots]^2$$

$$+[-A_1\cos\left(\frac{2\pi x}{d}+e_1\right)\sin(\kappa z-\mu_1 z)$$

$$+B_1\cos\left(\frac{2\pi x}{d}+e'\right)\cos(\kappa z-\mu_1 z)$$

$$-A_2\cos\left(\frac{2\pi x}{d}+e_2\right)\sin(\kappa z-\mu_2 z)+\ldots\ldots]^2.$$

In order to apply this result to our present question, it is supposed as a rough approximation that the terms with suffixes higher than one may be omitted. We thus obtain

$$A_0^2+\tfrac{1}{2}A_1^2+\tfrac{1}{2}B_1^2+2A_0A_1\cos\left(\frac{2\pi x}{d}+e_1\right)\cos(\kappa z-\mu_1 z)$$

$$+2A_0B_1\cos\left(\frac{2\pi x}{d}+e_1'\right)\sin(\kappa z-\mu_1 z)$$

$$+\tfrac{1}{2}A_1^2\cos\left(\frac{4\pi x}{d}+2e_1\right)+\tfrac{1}{2}B_1^2\cos\left(\frac{4\pi x}{d}+2e_1'\right),$$

which as a function of z is periodic with a period determined by $\kappa z-\mu_1 z=2\pi$, or

$$z=\frac{\lambda}{1-\sqrt{\left(1-\frac{\lambda^2}{d^2}\right)}}.$$

In the cases with which we are concerned λ^2 is small in comparison with d^2, so that approximately $z=2d^2/\lambda$. So far, then, as this theory extends, the phenomena behind the grating are reproduced with every retreat through a distance $2d^2/\lambda$; but, on account of the terms omitted, this conclusion does not apply to the subordinate periods (on which depends the performance of a copy in the spectra of higher order); nor does it apply rigorously even to the principal period itself.

Similar results to those given by direct inspection on the coarse grating have been obtained by photographic copying of finer ones, a lens (as already explained) being substituted for flat glass as a support for the sensitive film. When the copy is held so that the spectrum of the first order is formed upon the eye, several dark rings are visible, separated by intervals of brightness. With the 6000 Nobert the diameter of the first dark ring was ·54 inch, and at the centre round the point of contact there was a dark spot nearly as dark as the ring. In the second and third spectra the centres were also dusky, though not so black as in the first. The diameter of the first dark ring in the second spectrum was ·30 inch.

The occurrence of a dark centre is a point of interest, as showing that for purposes of reproduction it is possible for the

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contact to be too close, though I do not remember to have met with this in practice; and theoretically it is what would be expected when we consider that the original does not act by opacity. According to this view a different result should be obtained in copying an opaque grating; and such I have found to be the case. For this purpose I employed a copy of the same 6000 Nobert, taken some years ago on a tannin plate, and prepared the photographic film on the *same lens* as before. When the resulting photograph was examined, the spectra of the first three orders showed *bright centres*. The diameter of the first dark ring in the first spectrum was ·44 inch—smaller than before.

With the 3000 Nobert in place of the 6000 the ring-system is formed on a larger scale. The centres for the first four spectra are black, with the exception of the actual place of contact, where evidently the collodion film was impressed mechanically. The diameter of the first dark ring in the first spectrum is ·90 inch, not quite the double of ·54 inch, although the same lens as before was used. In the second spectrum the diameter of the first dark ring is ·56 inch, and in the third spectrum ·40 inch.

Interesting as these bands may be in theory, they are to be avoided as much as possible in the practical reproduction of gratings, not merely because a part of the area is lost, but also on account of the reversal which takes place at every revival of brightness. Without having examined the matter very closely, I had generally found the performance of gratings which showed these bands to be inferior; and now it would seem that the explanation is to be found in the above-mentioned reversals, which could not fail to interfere with the resolving-power.

During my early experiments it happened once that in the course of printing an accidental shifting took place, leading to the impression of a double image. A more perfect result was afterwards obtained by intentionally communicating to the plates a slight relative twist in the middle of the exposure. When a spectrum from such a grating is thrown upon the eye, parallel bars are seen perpendicular to the direction of the grooves; but the number and position of these bars depend upon the order of the spectrum. In one case twenty-five bars were counted in the first spectrum, and twice that number in the second. But it is unnecessary to dwell further upon these observations, as they correspond exactly to what the ordinary theory of gratings would lead us to expect.

January 29.

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XXVI. *On the Formation and Decomposition of Carbonic Acid.* By J. B. Lawes, *F.R.S.**

NO facts in science have been more clearly established than those which relate to the decomposition of carbonic acid by the green parts of plants (resulting in the fixation of the carbon and the return of the liberated oxygen to the atmosphere) and the part played by animal life and combustion in again uniting the carbon with the oxygen.

The vast quantity of carbon locked up in coal and other carbonaceous deposits leads to the conclusion that, at one period of the earth's existence, the amount of carbonic acid must have been larger than it is at the present time; but since the period when accurate determinations of carbonic acid were first made, no appreciable difference in the quantity has been shown to exist, although it would appear probable that the great increase of animal life, the destruction of forests, and the combustion of coal would tend to increase the amount.

The Rothamsted experiments which have been carried on for so long a period, while they clearly establish the fact that the atmosphere is the main, if not the exclusive, source of the carbon fixed by plants, at the same time point to conclusions which differ somewhat from those generally received, with regard to the action of man and animal life on one side, and that of plants on the other, in maintaining the equilibrium of the atmosphere.

I propose to take stock of the soil and atmosphere resting upon Great Britain, with the assumption (fortunately for us not a true one) that our atmosphere is as much a fixture as our soils, and is not constantly being exchanged with the atmosphere coming from other parts of the globe.

The sources of the carbon given off into the atmosphere as carbonic acid are three:—

(1) Those derived from the consumption of coal;
(2) From the imported products used as food or for combustion;
(3) From the products of our own soil.

The agricultural statistics make the area of Great Britain to consist of between 56 and 57 million acres. In order to reduce the amount of figures as much as possible, in adopting this estimate as my basis, I have given in the table below the figures which apply to each acre of the surface per annum.

The figures relating to the consumption of coal, which constitute far the largest item in the table, have been furnished

* Communicated by the Author.

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me through the kindness of Mr. Robert Hunt, of the Museum of Practical Geology; and although they somewhat exceed two tons per acre per annum, I have based my calculation on that amount.

The figures relating to imports are based upon the larger articles of food (such as live animals, meat, sugar, potatoes) and combustible substances (as tallow, petroleum, &c.).

With regard to the carbon given off by the consumption of home produce, so far as relates to the 33¾ million acres which constitute the cultivated area, my estimates are formed upon what I believe to be the average yield of the land; upon the remaining area, consisting of mountains, heath, and waste, the only basis for calculation is to be derived from the rental value of such land, and the amount of stock which it will carry.

Without going into the detail of the figures, I have given in the following table a summary of the whole.

Carbon given off as Carbonic Acid per acre per annum in Great Britain.

	lbs.
By coal	3942
By imported products	300
By home-grown products...	1275
Total	5517

We find, according to the more recent estimates, that the amount of carbon, as carbonic acid, resting upon an acre of ground is equal to about 14,000 lbs. Now the amount given off by the sources which I have mentioned above would more than double the carbonic acid in our atmosphere in three years, provided that no compensation took place! I propose to examine into what is the probable extent of the compensation which may come in to limit this large increase.

With regard to coal and imported products, there is very little compensation possible; but with the products grown upon our own soils, there is a continuous exchange going on between the atmosphere and the plant; and the question is on which side the balance lies.

I am disposed to think that, upon arable land (when kept in a uniform state of fertility), the amount of carbonic acid, fixed and exhaled, would be nearly the same, but that, wherever the fertility of the soil was diminished, there the carbonic acid given off would be in excess of that fixed.

In the ordinary case of a farmer selling a certain amount of the products of his land and consuming the residue, the saleable products, consisting of corn and meat, are burnt into

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carbonic acid, while the straw and other residue slowly assume the same form. But the food sold is derived from fertility stored up in the soil, accumulated by vegetation in ages long past, and thus goes also to swell the amount of carbonic acid contributed by the soil to the atmosphere.

This is made perfectly clear by long-continued experiments at Rothamsted, which show that crops raised on unmanured land, as also upon land receiving the mineral food of plants but no nitrogen, derive their nitrogen from the store in the soil; and as this nitrogen exists in combination with carbon, the latter is given off in the form of carbonic acid. Further, as agriculture is generally attended with more or less exhaustion of the soil, there is, in consequence, a slight increase in the carbonic acid added to the atmosphere by cultivation.

This is more clearly seen when we consider that most of the land which is now in cultivation was previously woodland or pasture. At Rothamsted we have found by analysis that the pasture contains more than twice as much organic carbon and nitrogen as the arable land—a result which may be attributed to the fact that on the latter these substances have been gradually destroyed or carried off by cultivation. On the other hand, in cases where arable land has been converted into permanent pasture, carbon again becomes fixed in the soil, both as roots in living vegetation and as organic carbon.

It is true that on land which receives a large dressing of dung annually we have found a very considerable increase of the carbon in the soil; in gardens also, in land laid down to pasture, and woodlands (especially newly planted woods) carbon would be abstracted from the air; but, generally speaking, I think that more carbon is given off from the products of the soil of Great Britain than is fixed by the living vegetation. If, in fact, we were entirely dependent on the atmosphere resting upon our country for existence we should very soon perish.

A process very similar to what I have described, though not to the same extent, is going on throughout the world: population and animals increase, fuel is burnt, forests are destroyed, and the stores of carbon and nitrogen accumulated in the soil by natural vegetation are slowly dissipated. We must look, therefore, to some other source, rather than to the land, for the restoration of the balance.

It is quite possible that this source is to be found in the ocean. According to analyses made by Dr. Frankland, the sea, even at a great depth, is very rich in organic carbon and nitrogen; it also contains large quantities of nitric acid; and these quantities are being increased from day to day by fresh

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supplies carried in by all the rivers of the globe. The tendency of the operation going on upon the land is to reduce the stock of carbon and increase that of carbonic acid,

(1) by the combustion of coal and other substances used as fuel;
(2) by the destruction of carbon accumulated near the surface of the soil by natural vegetation;
(3) by the increase of man and animals.

On the other hand, there may be a corresponding increase of organic carbon in the ocean, and in this way the balance may be kept up. Dr. Frankland's various analyses of sea-water, extending to a depth of between 700 and 800 fathoms, show that even at this depth, which is less than half of the estimated average, the amount of carbon, as organic carbon, is about three times as much as the carbon, as carbonic acid, in the atmosphere resting upon an equal area of surface. When we consider the immensity of the ocean, it is evident that the operations of animal and vegetable life in it must have a vast influence upon our atmosphere, of the value of which we appear to be altogether ignorant.

XXVII. *On the Rate of the Decrease of the Light given off by a Phosphorescent Surface.* *By* Lieut. L. Darwin, R.E.*

I CARRIED out a series of experiments at Chatham with the view of determining the law of the rate of decrease of the light of a phosphorescent surface. The experiments were conducted by comparing the light given off by a surface covered with Balmain's luminous paint, with a sheet of tissue paper illuminated from the further side by a Sugg's burner regulated to give about the light of four standard candles. The surface coated with paint was one side of a thin metal vessel, which was filled with a mixture of ice and water; the object of this was to keep the temperature as uniform as possible, as any increase of temperature increases the light given off by the paint. A sheet of tissue paper, of about the same size as the painted surface, was arranged just above it so that the light of the burner illuminated the tissue paper from the further side to the observer. The whole was enclosed in a box with an opening at one side, through which the light of the burner reached the tissue paper only, and opposite to it a small hole through which the observations were made. In this way only the light from the two surfaces

* Communicated by the Physical Society. Read December 11th.

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reached the eye, and the light of the room did not reach the surface of paint. Besides these reasons for observing through a small hole, it is better to do so, because the light given off by the illuminated tissue seems to vary with the angle at which it is observed, and the readings would not be constant if the eye were not in a fixed position. Experiment showed that the diffused light in the box from the illuminated tissue paper was not enough to have any effect on the phosphorescent paint. Both the paint and the tissue were observed through a solution of cupric sulphate, and blue glass; this did not, as far as I could see, cause any alteration in the colour of the paint. If the blue glass alone was used, the tissue looked pink in comparison with the paint; and with the cupric sulphate solution alone it looked green; but with the two the colour was so nearly imitated, that when the intensity of the light was the same I could only distinguish the two surfaces by their positions.

The illumination of the tissue was estimated by the distance of the burner from the tissue; this assumes that the light given off by the tissue varies directly as the light striking it on the reverse side. Bright sunlight was reflected onto the the painted surface by a mirror; and after it had acted for a few seconds, the room was darkened, with the exception of the light of the standard burner. For five minutes before this time, I took the precaution of hiding my eyes, so that no light could get at them. As soon after this as possible, observations were taken. Two slightly different methods were employed. At first the burner was placed at a definite distance from the tissue, and the time was noted at which the paint and tissue first appeared to be equally bright, and also that at which they first appeared to differ again in illumination; the mean of these two times was taken as the time of equal illumination. The burner was then moved away to a further fixed position, and another observation taken in the same manner. After five observations had been taken in this way, the rate of decrease of the light became very slow, and this method did not work well. The distance of the burner to obtain equal illumination was then found by moving it backwards and forwards, and noting the position at which the illumination of the two surfaces appeared the same; this was done four times as rapidly as possible, two before and two after the time at which an observation was wanted. The following table gives the mean of two series taken in this manner. The first column gives the illumination of the paint: the light given off by the tissue when the burner was nine inches from it is taken arbitrarily as 100; and the other illuminations are calculated accordingly

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from the distance of the burner. No absolute standard was attempted, as the light of the tissue would vary with its thickness, probably with the angle at which it was observed, and with the exactness with which the colour of the paint was imitated by means of the glass and solution. The second column gives the time at which the observation was taken.

TABLE.

Observed Light.	Observed Time.	Calculated Time.
	m. s.	m. s.
100·	0 0	0 0*
56·5	18	19
25·1	45	1 8
14·1	1 33	2 11
9·04	2 34	3 25
6·10	5 0	5 0*
2·89	10 0	9 59
1·34	20 0	19 53
·782	30 0	31 52
·604	40 0	39 41
·433	50 0	53 0
·376	60 0	60 0*
·285	80 0	76 14

I also made an attempt to calculate a curve which should as nearly as possible coincide with these results. The form of the curve which naturally suggested itself was $\frac{A}{t+B}=l^c$; t=time, l=light, A, B, and C being constants. A curve of this form to pass through three points (*) was calculated: A=26·1, B=·498 minute, C=·86. That is to say, the rate of decrease of the light varies as the light to the power of 1·86; and at that rate the light would have been infinitely great 30 seconds before the first observation. Taking the illuminations as obtained from the observations, the time of the observations was calculated according to the above formula; and the result is given in the third column. Three of the observed times differ from the calculated times more than can be accounted for by errors of observation; and it appears that the equation does not give quite the correct curve. From this and from another series of observations, it appears that in its lower parts the curve is very nearly of the form $\frac{A}{t+B}=l$; that is to say, that the rate of decrease of the light varies as the square of the light.

By comparing several series of observations it appears to

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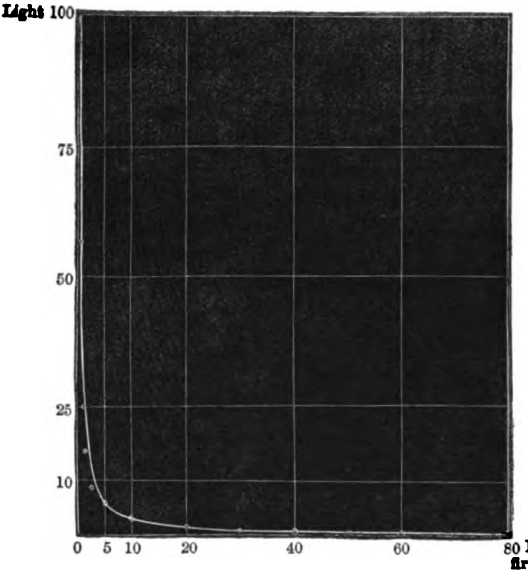
me that the rate of decrease is quite independent of the intensity of the first illumination.

Diagram showing the Rate of Decrease of the Light given off by Phosphorescent Paint.



o Observed illumination.

—— Curve calculated from the equation $y^{\cdot 36} = \frac{26 \cdot 1}{x + \cdot 498}$.

XXVIII. *Theory of Voltaic Action.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN stating* that the results obtained by me† are due to a difference of potential between the metal and its oxide, chloride, or sulphide, Professors Ayrton and Perry continue to ignore the concluding phase of these experiments, though I have brought it prominently forward in the 'Electrician' for December 4, from which they give a quotation. I refer to the fact that in the copper-iron ring experiments, though after admission of the hydrogen-sulphide gas the copper side becomes strongly positive to the iron, very soon this difference

* Phil. Mag. for Jan. 1881, p. 48.
† Phil. Mag. for Aug. 1878, p. 142, and Feb. 1879, p. 109.

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of potential *diminishes, and finally disappears* if the experiment be continued sufficiently long. This diminution is also seen in the copper-nickel experiment in hydrochloric acid gas. If the potential-difference is due to "contact action" between the metals, their sulphides, and the surrounding gas, why does it disappear while all these conditions are still present? Its disappearance seems to me to be obviously due to the cessation of chemical action; and even its diminution under these circumstances appears to completely dispose of the assumption that the effect is due to such "contact action." From the statement that my zinc plates "were coated with zinc sulphide . . . . . or zinc chloride," one is with regret led to infer that Professors Ayrton and Perry have not paid much attention to the work they criticise. No zinc was used in the experiments with sulphur and chlorine compounds.

It is needless to occupy space by repeating my communication to the 'Electrician' respecting Dr. Exner's view of the voltaic theory and my own; but I am not conscious of having made the mistake attributed to me at p. 46. I said nothing about the "actual electric charge" in the oxide film, gas film, or platinum, and was quite aware it is not measured by Kohlrausch's method.

Respecting Pfaff's work, it is well to remember that, according to De la Rive*, the electricity produced by the oxidation of ·00000000002 gramme of zinc is sufficient, with the help of a condenser, to cause divergence of the gold leaves of an electroscope. Did Pfaff succeed in keeping his apparatus free from quantities of oxygen of this order? Even the Crookes's vacuum in Professors Ayrton and Perry's proposed experiments may not give any but negative results. In such experiments "the gas film previously condensed on the metal is not removed, since it is held fast by a force far greater than the pressure of an atmosphere"†.

The extreme difficulty of getting rid of occluded or condensed gas from the metallic electrodes of highly exhausted tubes is well known‡.

Yours truly,

J. Brown.

Belfast, Feb. 12, 1881.

* *Traité d'Electricité*, t. ii. p. 777.
† Wiedemann, *Galvanismus*, i. p. 13.
‡ See Hittorf, Wied. *Ann.* 1879, No. 8, p. 574.

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XXIX. *On Images formed without Reflection or Refraction.* *By* LORD RAYLEIGH, *F.R.S., Professor of Experimental Physics in the University of Cambridge**.

THE function of a lens in forming an image is to compensate by its variable thickness the differences in phase which would otherwise exist between secondary waves arriving at the focal point from various parts of the aperture. If we suppose the diameter of the lens ($2r$) to be given, and its focal length f gradually to increase, these differences of phase at the image of an infinitely distant luminous point diminish without limit. When f attains a certain value, say f_1, the extreme error of phase to be compensated falls to $\frac{1}{4}\lambda$. Now, as I have shown on a previous occasion†, an extreme error of phase amounting to $\frac{1}{4}\lambda$, or less, produces no appreciable deterioration in the definition; so that from this point onwards the lens is useless, as only improving an image already sensibly as perfect as the aperture admits of. Throughout the operation of increasing the focal length, the resolving-power of the instrument, which depends only upon the aperture, remains unchanged; and we thus arrive at the rather startling conclusion that a telescope of any degree of resolving-power might be constructed without an object-glass, if only there were no limit to the admissible focal length. This last proviso, however, as we shall see, takes away almost all practical importance from the proposition.

To get an idea of the magnitudes of the quantities involved, let us take the case of an aperture of $\frac{1}{5}$ inch, about that of the pupil of the eye. The distance f_1, which the actual focal length must exceed, is given by

$$\sqrt{\{f_1^2+r^2\}}-f_1=\tfrac{1}{4}\lambda;$$

so that

$$f_1=\frac{2r^2}{\lambda}.$$

Thus, if $\lambda=\frac{1}{40000}$, $r=\frac{1}{10}$, $\quad f_1=800.$

The image of the sun thrown on a screen at a distance exceeding 66 feet, through a hole $\frac{1}{5}$ inch in diameter, is therefore at least as well defined as that seen direct. In practice it would be better defined, as the direct image is far from perfect. If the image on the screen be regarded from a distance f_1, it will appear of its natural angular magnitude. Seen from a dis-

* Communicated by the Author.

† Phil. Mag. November 1879.

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tance less than f_1, it will appear magnified. Inasmuch as the arrangement affords a view of the sun with full definition and with an increased apparent magnitude, the name of a telescope can hardly be denied to it.

As the minimum focal length increases with the square of the aperture, a quite impracticable distance would be required to rival the resolving-power of a modern telescope. Even for an aperture of four inches f_1 would be five miles.

A similar argument to that just employed to find at what point a lens begins to have an advantage over a simple aperture, may be applied to determine at what point an achromatic lens begins to assert a perceptible superiority over a single lens in forming a white image. The question in any case is simply whether, when the adjustment is correct for the central rays of the spectrum, the error of phase for the most extreme rays (which it is necessary to consider) amounts to a quarter of a wave-length. If not, the substitution of an achromatic lens will be of no advantage.

If μ be the refractive index for which the adjustment is perfect, then the error of phase for the ray of index $\mu+\delta\mu$ is $\delta\mu \,.\, t$, where t is the "thickness" of the lens. Now

$$(\mu-1)t=\frac{r^2}{2f};$$

so that, if the error of phase amount to $\frac{1}{4}\lambda$,

$$\frac{\delta\mu}{\mu-1}=\frac{\lambda f_1}{2r^2}.$$

In order to apply this numerically, let us take the case of hard crown-glass, for which the indices are given by Hopkinson*. The practical limits of the spectrum being taken at B and G, we have $\mu_B=1{\cdot}5136$, $\mu_G=1{\cdot}5284$, the difference of which is ·0148. If the focus be correct for the mean value of μ, the extreme value of $\delta\mu$ is ·0074, and that of $\delta\mu/(\mu-1)$ is ·0074/·521, or ·0142. In strictness we ought to take into account the variation of λ; but for such a purpose as the present we may put it at $\frac{1}{40000}$ inch; and then the fraction ·0142 expresses the admissible focus when a single lens is used as compared with the focus necessary when a lens is dispensed with altogether. Thus, if the aperture be one fifth of an inch, an achromatic lens has no advantage over a single one, if the focal length be greater than about 11 inches. If, on the other hand, we suppose the focal length to be 66 feet, a single lens is practically perfect up to an aperture of 1·7 inch. The effect of spherical aberration in

* Proc. Roy. Soc. 1877.

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disturbing definition was considered in my former paper. In such a case as that last specified it is altogether negligible. The advantage of a long focus was well understood by Huyghens and his contemporaries; but it may have been worth while to consider the matter for a moment from another point of view, from which it clearly appears that the substitution of an achromatic for a single lens serves no other purpose than to diminish the minimum admissible focal length.

Returning now to homogeneous light, let us consider the case of an *annular* aperture of radii r_1 and r_2. The extreme difference of phase at distance f is now $(r_2^2-r_1^2)\div 2f$. If this be $\frac{1}{4}\lambda$, we get

$$f_1=\frac{2(r_2^2-r_1^2)}{\lambda}=\frac{2(r_2+r_1)(r_2-r_1)}{\lambda}$$

as the value of the minimum distance at which a lens can be dispensed with without loss. If r_2-r_1 be small, f_1 is much smaller than for a full circle of radius r_2; and it might appear that a great advantage would be gained either in the diminution of f_1 or by an increase in r_2. The question, however, remains whether *with a lens* the definition due to an annular aperture of given outer radius r_2 is independent of the inner radius r_1.

The image of a mathematical point consists, it is known, of a central patch of brightness, surrounded by rings alternately dark and bright. If we conceive the radius of the central stop (i. e. r_1) gradually to increase from 0 to r_2, the diameter of the central luminous patch diminishes in the ratio 3·83 : 2·41. From this it might be supposed that the definition due to the marginal rim acting alone would be superior to the definition due to the whole aperture*. It is true that there is at first some improvement in definition; but as r_1 approaches equality with r_2 a rapid deterioration sets in, notwithstanding the smallness of the central luminous patch. In order to understand this it is necessary to examine more minutely the distribution of light over the entire field.

If the point under consideration be distant ρ from the centre of the diffraction-pattern, the illumination for the full aperture is given by

$$\frac{\pi^2r^4}{\lambda^2f^2}\left[\frac{2J_1\left(2\pi\frac{r\rho}{\lambda f}\right)}{2\pi\frac{r\rho}{\lambda f}}\right]^2=\frac{\pi^2r^4}{\lambda^2f^2}\,\frac{4J_1^2(y)}{y^2},$$

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* ... Diffraction of Object-glasses (Astr. Month. Notices, 187...

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if $y=2\pi\frac{r\rho}{\lambda f}$, J_1 being the symbol of the Bessel's function of order unity. The dark rings correspond to the roots of J_1, and occur when $y=3{\cdot}83$, $7{\cdot}02$, $10{\cdot}17$, &c.

The whole illumination within the area of the circle of radius ρ is given by

$$\int I^2 2\pi\rho d\rho = 2\pi r^2 \int_0^y y^{-1} J_1^2(y) dy.$$

This integral may be transformed by known properties of Bessel's functions. Thus*,

$$\frac{J_1(y)}{y} = J_0(y) - \frac{dJ_1(y)}{dy};$$

so that

$$\frac{J_1^2(y)}{y} = J_0(y) \cdot J_1(y) - J_1(y)\frac{dJ_1(y)}{dy}$$

$$= -J_0(y)\frac{dJ_0(y)}{dy} - J_1(y)\frac{dJ_1(y)}{dy}.$$

We therefore obtain

$$2\int_0^y y^{-1} J_1^2(y) dy = 1 - J_0^2(y) - J_1^2(y).$$

If y be infinite, $J_0(y)$ and $J_1(y)$ vanish, and the whole illumination is expressed by πr^2, as is evident *à priori*. In general the proportion of the whole illumination to be found *outside* the circle of radius ρ is given by

$$J_0^2(y) + J_1^2(y).$$

For the dark rings $J_1(y)=0$; so that the fraction of illumination outside any dark ring is simply $J_0^2(y)$. Thus, for the 1st, 2nd, 3rd, and 4th dark rings we get respectively ·161, ·090, ·062, and ·047, showing that more than $\frac{9}{10}$ of the whole light is concentrated within the area of the second dark ring.

The corresponding results for a narrow annular aperture would be very different, as we may easily convince ourselves. The illumination at any point of the central spot or of any of the bright rings is proportional to the *square* of the width of the annulus, while the whole quantity of light is proportional to the width itself. As, therefore, the annulus narrows, a less and less proportion of the whole light is contained in any finite number of luminous rings, and the definition of an image cor-

* Todhunter's Laplace's Functions, p. 297.

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responding to an assemblage of luminous points is proportionally impaired.

The truth is that, so far as it is possible to lay down any general law at all, the definition depends rather upon the *area* than upon the *external diameter* of the aperture. If A be this area, the illumination at the focal point, where all the secondary waves concur in phase, is given by $I_0^2 = A^2/\lambda^2 f^2$, the primary illumination being taken as unity. The whole illumination passing the aperture is on the same scale represented by A. Hence if A′ be the area over which an illumination I_0^2 would give the actual total illumination, $AA' = \lambda^2 f^2$; and A′, being in some sense the area of the diffraction-pattern, may be taken as a criterion of the definition.

In the case of an annulus we saw that the minimum focal length allowing a lens to be dispensed with is also dependent upon the *area* of aperture—$\pi(r_2^2 - r_1^2)$; so that it would appear that if the object be to form at a given distance, and without a lens, as well-defined an image as may be, it is of comparatively little consequence whether or not an annular aperture be adopted. A moderate central stop would doubtless be attended with benefit; but it is probable that harm rather than good would result from any thing like extreme proportions.

January 29.

P.S.—Reference should be made to a paper by Petzval on the Camera Obscura (Phil. Mag. Jan. 1859), in which the definition of images formed without lenses is considered. The point of view is different from that above adopted.

February 18.

XXX. *On Action at a Distance.* *By* S. Tolver Preston*.

ALTHOUGH I am far from admitting the propositions contained in Mr. Walter R. Browne's recent reply and previous article (as he seems to assume), some of which appear to partake somewhat of the nature of assertion, I will nevertheless notice one or two points in his last communication, as illustrative of the paradoxical kind of reasoning employed by those who attempt to support "action at a distance."

On page 130 of the last Number of the Philosophical Magazine, Mr. Browne remarks, "Nothing is fully explained until it has been brought under an inexplicable law." This "inexplicable law" is exemplified by Mr. Browne's theory of "action at a distance" as applied to gravity—which accordingly,

* Communicated by the Author.

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he assumes, *explains* gravity. Gravity as we know, is found to vary as the *square* of the distance (and not in some other ratio). This peculiar circumstance is supposed by Mr. Browne to be *explained* by merely stating it to be a fact. What would be thought of any one saying that the "law" of light, which also varies in intensity as the *square* of the distance, were *explained*, without considering the existence of the æther, or the physical basis of light—without reflecting on the fact that the area of the spherical wave over which the energy is spread varies as the *square* of the distance (and not in some other ratio). By a knowledge of the physical conditions determining light, the fact that the intensity of light varies as the *square* of the distance admits of being predicted beforehand. So the "law" of gravity will admit of being predicted beforehand from the determining physical conditions, as soon as these are known. This capability of prediction beforehand, by a knowledge of the physical conditions, is one test of the *explanation* of a physical fact. Yet some would teach us to swallow the bare *statement* that gravity varies as the *square* of the distance, as an *explanation*. The same may be said of magnetism (which will never be *explained* until the determining physical conditions are known)*. Surely no argument is necessary to establish such points as these, which (as matters of scientific principle) should really permit no difference of opinion, were it not for the absence of the "competent faculty of thinking," so strongly insisted on by Newton,—or absence of due reflection. It is needless to lay stress on the fact that an intellect which *on the whole* may be more competent than another, may yet be particularly incompetent in some given direction (and perhaps in this one). For minds are so differently constituted.

Finally, Mr. Browne appears to find difficulty in realizing the fact that if we take ultimate *atoms*, and if we suppose the atoms of a bar of iron to be *elastic* (as according to the modern theory of matter, for example), and, finally, if these atoms be of open structure (say something of a ring form), then they can obviously *bend* when the bar is compressed; so that it is not necessary to assume the constituent atoms of the bar to be normally at a distance from each other, in order to explain the contraction of the bar (within certain limits) under an applied pressure. It is so far evident that mechanical considerations are independent of scale, and atoms of

* It seems to have been overlooked by Mr. Browne, that no doubt is now entertained by competent judges as to the essentially physical nature of magnetism, as it appears that there are grounds for believing that the velocity of propagation of the magnetic influence has been approximately determined.

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open structure might be conceived to be in contact, and yet the whole capable of compression (much like a system of elastic springs in contact).

February 8, 1881.

XXXI. *On Action at a Distance.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT was with some surprise that I read in your last issue Mr. Browne's reply to a note of mine which you published in your January number. The note contained a proof (a perfectly valid and conclusive proof, I believe) that the direct action of one body on another at a distance was incompatible with accepted physical axioms, viz. Newton's third law and the law of the conservation of energy. In so far as it was an answer to Mr. Browne's paper with the same heading in your December number, it was written under the impression that there could be no possible difference between us with respect to the elementary principles of mechanics. I now find, however, that he appears to disbelieve in the axiom that reaction is equal to action, and that he ignores that particular kind of reaction which is against acceleration. These excentricities make it difficult to argue with him. But these are not the worst; for he actually takes the trouble to invent and publish a solemn argument to prove that no work can be done by one body on another when they are both moving at the same rate in the same direction. How any one who has ever seen a horse drawing a cart, or a nurse pushing a perambulator, can fail to see through this transparent absurdity, I am really unable to conceive.

It is not unnatural, perhaps, that Mr. Browne should set me down as "fatally wrong" and "obviously in error," seeing that he has wholly failed to understand my argument; and I can only regard his letter as an instructive example of how possible it is for a man to be familiar with the mere mathematical methods and reasonings connected with a subject, while yet he is in a direful fog regarding the simple and fundamental principles of the subject itself, on which nevertheless the higher reasonings must in reality rest.

I have the honour to remain, Gentlemen,

Your obedient Servant,

OLIVER J. LODGE.

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XXXII. *Intelligence and Miscellaneous Articles.*

ON SOME EXPERIMENTS INSTITUTED TO SUPPLY ALL THE LINES TERMINATING AT THE CALCUTTA TELEGRAPH-OFFICE WITH CURRENTS TAPPED FROM THE MAIN CURRENT PRODUCED BY A DYNAMO-ELECTRIC MACHINE. BY LOUIS SCHWENDLER, ESQ., M.I.C.E., ETC.

MR. SCHWENDLER gave an outline of his paper explaining the latest experiments he had made in order to prove the practicability of his new method of supplying signalling currents. This method was published in the Journal of the Asiatic Society, part 2, vol. xlix. 1880, and in the Philosophical Magazine, No. 52, December 1879, Supplement. After certain incidental delays, the final trial came off on Sunday the 29th August, 1880.

One of the dynamo-electric machines employed for lighting the sheds at Howrah Railway-station, produced the required strong main current; and the signal currents were conveyed from Howrah to the Calcutta Telegraph Office by an ordinary telegraph-line about 2 miles in length.

In all eleven long main circuits were supplied with telegraph-currents in this manner, viz. the lines to Bombay, Madras, Kurrachee, and Rangoon. The currents sent from Calcutta and the currents received at the out-stations were measured; and by it the satisfactory result was established that in all the lines, even in the longest, the tapped or machine currents were considerably stronger than the currents produced by the ordinary signalling-batteries at present in use.

The trial lasted for three hours, under the direct supervision of Mr. C. B. P. Gordon, the Superintendent of the Bengal Division, who carefully watched the working of the office. The traffic was despatched with regularity in the ordinary manner.

At the beginning of the trial the main current was produced through an iron wire of 0·21″ diameter, offering a resistance of 1·5 B.A. unit. This main current was carefully measured, and found to be equal to 36801 millicersteds. At the end of the trial the main current was produced through the arc of an electric lamp, giving a light of about 6000 standard candles when measured under an angle of 45° with the horizon. In this latter case the main current amounted to 45706 millicersteds *.

The total current tapped from the main current (when all the eleven lines were simultaneously sending) equalled 129 millicersteds, quite an insignificant fraction of the large main current.

Mr. Schwendler stated that this practical trial of the method had shown that it was perfectly reliable, and no practical telegraph-engineer would doubt that it was very convenient to produce the currents in this manner. It was therefore under consideration to introduce the system at the Calcutta office. Mr. Schwendler concluded his paper with some suggestions for utilizing the larger portion of the current not required for telegraph-purposes, saying:—

* One cersted is equal to one weber per second. One millicersted is equal to one milliweber per second.

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"The useful work for the main current at night would most conveniently take the shape of an electric light to illuminate very efficiently the Signal-office. The electric light would produce at least 50 times less heat than if the same quantity of light were obtained by combustion; and this is no doubt a great advantage in a hot climate like that of India. During the day-time the main current might be used for pulling the punkhas, lifting messages, or, more generally, for working a pneumatic system of despatching messages between the head telegraph-office and local centres in Calcutta. If Calcutta had the good fortune to possess a colder climate, it might be suggested to use the heat developed by the main current in a coil of iron wire for warming rooms. It would then only be necessary to lead the wire along the walls in a manner similar to that in which rooms are often heated by hot-water pipes; only the electrical method would be far more economical. The quantity of heat given out by such a wire is by no means small. In one case it was equal to 20473 Ω ergs per second, equal to 488 grm. degree-centigrade per second. This is about equal to the heat produced by an ordinary German stove consuming 6 lbs. of coals per hour, supposing that the loss of heat when coals burn under a steam-boiler is about four times greater than when they burn in a German stove. It appears therefore that the heat emanating from the wire should suffice to keep a moderate-sized and ordinarily ventilated room at a comfortable temperature even when situated in the highest latitude."—*Proceedings, Asiatic Society of Bengal, for November* 1880.

DETERMINATION OF THE COLOURS CORRESPONDING TO THE FUNDAMENTAL SENSATIONS BY MEANS OF ROTATING DISKS. BY M. A. ROSENSTIEHL.

As Maxwell has shown*, rotating disks permit us to determine with great precision the laws of colour-vision. I am going to furnish fresh proofs of that fact by showing how with their aid one can find exactly the position occupied in the chromatic circle by the three colours which, according to the theory of Young, correspond to the fundamental sensations.

I have taken as starting-point a chromatic circle executed on sheets of paper with covering colouring-matters. The whole forms a continuous succession of seventy-two colours, in which the red, the yellow, the blue are at equal distances from one another, and the intervals are filled by colours also as equidistant as possible to the eye, with the same height of tone, and all equally clear.

Each colour can be regarded as resulting from the mixture of two others (I mean the mixture of the sensations). I proposed to myself to measure the two components of each of them.

Example.—Let us suppose that orange is a mixture of red and yellow. I compose a disk of two concentric circles. The smallest is formed by two sectors—one orange, the other blue; the largest comprises a red sector, a white sector, and a void representing the absence of light. I set it in rapid rotation. The two circles have an identical appearance if the angles of the different sectors are

* Trans. Roy. Soc. Edinb. vol. xxi. pp. 275–298.

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well chosen. The blue sector extinguishes the yellow of the orange and forms white; the red alone remains. The measure of the two resulting sensations is given by the angles of the red and white sectors. I found

$$140° \text{ orange} + 220° \text{ blue} = 281° \text{ red} + 56° \text{ white.}$$

All the colours of the circle have been thus studied with respect to two pairs of complementary colours—yellow and blue, red and the fourth green (the first three because they are the primary colours of artists; the last serves as the auxiliary colour).

The chromatic circle was thus found to be divided into four sections:—

The red was measured	from the blue to the red, extinguishing the blue by the yellow. from the red to the yellow, extinguishing the yellow by some blue.
The yellow was measured	from the red to the yellow, extinguishing the red by the fourth green. from the yellow to the fourth green, extinguishing the green by red.
The fourth green was measured	from the yellow to the fourth green, extinguishing the yellow by blue. from the fourth green to the blue, extinguishing the blue by the yellow.
The blue was measured	from the fourth green to the blue, extinguishing the green by the red. from the blue to the red, extinguishing the red by the fourth green.

The following are the results obtained*:—

1. The line which represents the proportion of the extreme sensations in the intermediate colours is a straight line; therefore the intermediate colours are, to the eye, rigorously equidistant.

2. The line representing the sensation of yellow reaches its culminating point upon the ordinate which corresponds to yellow.

3. This case, which I thought must be general for the four lines, forms, on the contrary, the exception: for the other colours things take place in a different manner. The sensation of red goes on increasing in a straight line, from the blue to the red; and beyond this it continues to rise without deviation till it comes to the orange, where it attains its culminating point, to sink afterwards till it reaches the yellow, where it is zero.

In like manner the sensation of green attains its maximum in the third green-yellow, and that of the blue in the third blue.

The signification of these remarkable facts does not come out well unless we look at the experiment itself which has revealed the existence of the three maxima. The fact is as follows:—

One and the same blue, the sensation of which is mingled with that of the orange on one side and that of the second and third green-yellow on the other, produces with the first a superior sensation of red to that produced by the sight of the red of the circle,

* *Journal de Physique*, t. vii. p. 16.

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and with the second the sensation of green to a higher degree than is done by the sight of the green itself.

This blue, however, still does not represent the sensation of that colour in its greatest intensity, since a better is obtained by mingling the sensations of the fourth green and the third blue. The orange, the second or third green-yellow, and the third blue are therefore three points of the chromatic circle which possess for our eye peculiar properties, since they permit the reproduction, by the mixture of their sensations, of the most intense red, green, and blue.

These three colours possess the properties of the fundamental sensations of Young. But can they really be regarded as representing exactly those three points? The theory of Young is only an hypothesis; and physicists have not come to a determination upon the true position of these three sensations. The uncertainty which prevails on the question causes Helmholtz* to say:—"The choice of the fundamental sensations presents at first something arbitrary. One might choose at pleasure three colours whose mixture produces white . . . . . There does not yet exist, to my knowledge, any means of determining the fundamental colours but the examination of persons affected with dyschromatopsy."

I purpose to demonstrate, in a second Note, that, on the contrary, the colours in question are situated in the chromatic circle between very narrow limits, and that the three maxima obtained by the study of which I have here given a summary are in reality the colours corresponding to the fundamental sensations.—*Comptes Rendus de l'Académie des Sciences*, t. xcii. pp. 244–247, Jan. 31, 1881.

ON THE LIBERATION OF ELECTRICITY IN TOURMALINE BY PRESSURE. BY MM. JACQUES AND PIERRE CURIE.

We will at once enunciate the laws which follow from our experiments on the liberation of electricity in tourmaline by pressure. We will afterwards explain, with the rapidity required by the brevity of this Note, our experimental processes and the limits within which we have verified those laws.

I. *The two extremities of a tourmaline liberate electricity of opposite signs, in quantities equal to one another.*

II. *The amount set free by a certain augmentation of pressure is of opposite sign, and equal, to that produced by an equal diminution of pressure.*

III. *This quantity is proportional to the alteration of pressure.*

IV. *It is independent of the length of the tourmaline.*

V. *For one and the same alteration of pressure per unit of surface, it is proportional to the surface.*

The direct result of the experiments from which laws IV. and V. are deduced can be stated in a simple manner thus:—*For one and the same alteration of pressure, the quantity of electricity set free is independent of the dimensions of the tourmaline.*

The tourmalines which we wished to study had the form of prisms parallel to the principal axis. The two bases were covered with two sheets of tin foil protected outside by two thick plates of glass,

* *Optique Physiologique*, p. 384.

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between which the crystal was compressed by means of a solid wooden lever. One of the tin foils being in communication with the earth, the other was connected with the needle of a Thomson-Mascart electrometer. The deflection obtained after one variation of pressure was proportional to the quantity of electricity liberated —the capacity of the tin foil, under the conditions just now described, being always negligible in comparison with the capacity of the electrometer.

Tourmalines which are transparent, colourless or slightly coloured green, yellow, or rose, are in general perfectly insulating; and such only were employed for the quantitative experiments. Whatever may be their tint, these tourmalines seem to be nearly equivalent in regard to the electrical phenomena; the differences, if there are any, are certainly very slight; still it would be necessary to examine a considerable number of specimens before we could affirm that it is always so.

More or less opaque or black tourmalines are conductors of electricity. A black tourmaline gave the electrometer-needle an impulse equal to about one fifth of the deflection obtained for the same weight with a transparent tourmaline; moreover the needle returned rapidly to zero.

The deflections the equality or proportionality of which it was necessary to verify, considering the neglected causes of error, were exact only to within a twentieth part of their value. We have not judged it necessary to attempt to attain a closer approximation; for the accuracy of the laws enunciated results from the considerable differences between the dimensions of the tourmalines employed.

For one and the same surface the lengths varied from 0·5 millim. to 15 millims., therefore in the proportion of 1 to 30. For one and the same length the surfaces varied from 2 square millims. to 1 square centim., therefore in the proportion of 1 to 50. Given, therefore, the degree of approximation of the experiments, and supposing the laws enunciated to be limiting laws, we can certify that when the length is doubled the difference from the true law is less than a six-hundredth, and when the surface is doubled the difference is less than a thousandth.

A piece measuring 1 cubic millim. sets free, for one and the same pressure, the same amount of electricity as a piece of several centims. volume. Finally, the effect produced by the addition of one of the first kilograms is sensibly the same as that produced by the hundredth kilogram, for a surface of 1 square centim.

Gaugain, in a remarkable memoir, has shown the simplicity of the pyroelectrical phenomena of tourmaline. The laws enunciated by him may be placed over against those which form the subject of this Note. It is easy to see that they can be traced one upon the other, if we permit ourselves to be guided by the hypothesis that the phenomena resulting from variations of pressure and those resulting from variations of temperature are due to one and the same cause—namely, the contraction or dilatation in the direction of the axis of the tourmaline.—*Comptes Rendus de l'Académie des Sciences*, 1881, No. 4, t. xcii. pp. 186–188.

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CONFIRMATION OF THE NEBULAR HYPOTHESIS.

BY PLINY EARLE CHASE, LL.D.

Herschel's "subsidence" hypothesis is corroborated by the following extensions of Laplace's satellite-harmonics, from Jupiter to Earth:—

1. Earth's accelerated rotation : the acceleration by condensation to a centre of linear oscillation :: Jupiter's radius of accelerated rotation : Callisto's (Sat. IV) radius of accelerated rotation.

$$366{\cdot}2565 : 9 :: 4332{\cdot}5848^{\frac{2}{3}} : 16{\cdot}6891^{\frac{2}{3}}.$$

Searle gives 16·689 for Callisto's period; Newcomb and Holden give 16·7535524.

2. Jupiter's mean perihelion distance from Earth (4·978245 − 1, according to Stockwell) : Callisto's mean distance from Jupiter :: Jupiter's mass : Earth's mass.

$$3{\cdot}978245 : {\cdot}012585 :: \frac{1}{1047{\cdot}879} : \frac{1}{331245}.$$

If we take $g = 32{\cdot}088$ feet, Earth's isochronous radius (satellite-period = 1 year) is

$$\left(\frac{31558149 \text{ sec.}}{5073{\cdot}6 \text{ sec.}}\right)^{\frac{2}{3}} \times 3962{\cdot}8 = 1340291{\cdot}5 \text{ miles.}$$

The equation, $m^{\frac{1}{3}} \times$ isochronous radius $= \rho$, gives

$$331245^{\frac{1}{3}} \times 1340291{\cdot}5 \text{ miles} = 92736000 \text{ miles.}$$

The reason for these harmonics is to be found in the fact that Earth is the centre of the belt of greatest condensation, while Jupiter's mean aphelion is central between Neptune and Uranus at their opposition.

Haverford College, Feb. 8, 1881.

ON THE CHOICE OF THE UNIT OF FORCE IN ABSOLUTE ELECTRIC MEASUREMENTS. BY M. LIPPMANN.

It is known that the electric measurements called *absolute* rest upon the choice of the three units employed to measure the times, lengths, and forces; and it will be remembered that in 1863 a Committee of the British Association proposed to take for the unit of force the *dyne*—that is to say, the force capable of impressing on the mass of 1 gram, at the end of 1 second, a velocity of 1 centim. per second. Is the dyne in every respect the most advantageous that could have been chosen? Without wishing to discuss here that complex question completely, I may at least facilitate its solution by the following remarks, which have not, perhaps, been made in a sufficiently explicit and general manner by authors:—

1. *The absolute electromagnetic units of electrical resistance and capacity are independent of the choice of the unit of force**. Now

* In fact, let L, T, F be the length, time, and force, measured with any units. The quantity of magnetism μ is given by an equation of the form

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these are the only absolute units represented by standards. Therefore the unit of force might be varied at pleasure; and the ohms and microfarads at present distributed to the laboratories would no less preserve their value.

2. *The formulæ which permit passing from the absolute electromagnetic to the absolute electrostatic system, or inversely, are all independent of the choice of the unit of force.* This can be ascertained by glancing at the last line of the table given below, which it is easy to calculate. Now these formulæ are the only ones of this kind having a theoretic interpretation.

The units of intensity and electromotive force, on the contrary, depend on the choice of the unit of force. If, for example, it were thought necessary to substitute for the dyne another unit, the necessary operations for measuring these quantities would not be changed, but their numerical result would be enunciated differently: the volt and the weber would have to be replaced by other denominations; for they are mere denominations, not standards.

Besides, we can pass from one system of units to any other with the help of the following table:—

System.	Intensity.	Resistance.	Electromotive force.	Capacity.
Electromagnetic ..	$i=F^{\frac{1}{2}}$	$r=\frac{L}{T}$	$e=\frac{LF^{\frac{1}{2}}}{T}$	$c=\frac{T^2}{L}$
Electrostatic	$I=\frac{LF^{\frac{1}{2}}}{T}$	$R=\frac{T}{L}$	$E=F^{\frac{1}{2}}$	$C=L$
Ratios	$\frac{I}{i}=\frac{L}{T}$	$\frac{R}{r}=\frac{T^2}{L^2}$	$\frac{E}{e}=\frac{T}{L}$	$\frac{C}{c}=\frac{L^2}{T^2}$

Of course the analogous formulæ given by authors are equivalent to those of this table; but they are rather less simple, and they suppose the acceleration to be taken as the measure of the force.

The remarks just made show that the choice of the unit of force has not the importance which one would at first be tempted to attribute to it, and consequently that it would be easy to change that unit.

This being admitted, one may ask why the eminent authors of

$\frac{\mu^2}{L^2}=F$; μ is therefore of the degree $\mu=LF^{\frac{1}{2}}$. The intensity i of the current is given by Ampère's formula, which is of the degree $\frac{\mu i}{L}=F$, whence $i=\frac{LF}{\mu}=F^{\frac{1}{2}}$. Finally, the electrical resistance r is given by the equation $ri^2T=FL$, the two members of which represent respectively the electrical and the mechanical work; hence $r=\frac{FL}{i^2T}$; that is to say, $r=\frac{L}{T}$; r is therefore independent of F, which was to be demonstrated. In like manner, the electrical capacity c, derived from the charge iT by referring it ot the electromotive force $e=ri$, is of the form $\frac{iT}{ri}=\frac{T}{r}$. We have therefore

$$c=\frac{T^2}{L}.$$

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the electromagnetic system thought it necessary to measure the forces in dynes. The reason is simple: it is because, for the purpose of determining the couple of the forces exerted by the earth's magnetism upon a magnetized needle, those physicists have employed the method of oscillations. When this method is employed, what is found by experiment is an acceleration; it is then natural to take the accelerations for the measurement of the forces which produce them. The dyne has the advantage of greater simplicity in the case of the oscillation-method, and in that case only. Now it is very necessary to remark that the oscillation method is only an indirect means; for to measure a static couple it would have been more *direct* to employ the bifilar suspension of Gauss, or, more generally, a static method. But then no acceleration occurs: one has only to weigh the magnetized needle; the amount of the force is deduced from this weight, and is obtained directly in grams. It is circuitous and a complication to express it in dynes.

The dyne system moreover complicates considerably the transformation-formulæ; in what concerns the measurement of lengths it causes the advantages of the decimal system to vanish. Electricians have employed as the unit of length, some the metre, others the centimetre or the millimetre. Let us suppose one wishes to pass from one of these units to the other. In the dyne system, or, more generally, in the systems in which the force is defined by means of an acceleration, the units of intensity and electromotive force depend on the square root and the $\frac{3}{2}$ power of the unit of length; so that one is obliged to divide by a factor which must be calculated, and which is either the square root or the $\frac{3}{2}$ power of a power of 10. If, on the contrary, a weight or any other force independent of the unit of length be taken, the unit of intensity is independent of the unit of length. As to the unit of electromotive force, it, like the other electrical units, is of the first degree with respect to the lengths; and the result is that we can pass from the metre to the centimetre by merely transposing the decimal-point, an operation which can be done mentally: this is the essential advantage of the decimal system; and we give up profiting by it when we adopt the system of English units.

In short, the electrical standards and the principal theoretic formulæ being independent of the choice of the unit of force, the choice of that unit does not possess a very great importance, and its change is always easy. The dyne presents no essential advantage in any case. Finally, it is highly desirable that the units employed to measure the times, the lengths, and the forces should be the same in electricity as in all other departments of Physics. This unification, the aim of the excellent labours of the physicists of the British Association, has not been attained, as, ever since 1863, physicists have not come to an agreement to adopt the dyne. Perhaps the purpose would be more easily attained by proposing to electricians to take the second, the metre, and the weight of the gram as fundamental units—that is to say, the now so widely spread units of our metric system.—*Comptes Rendus de l'Académie des Sciences*, Jan. 24, 1881, t. xcii. pp. 183–186.

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THE

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[FIFTH SERIES.]

APRIL 1881.

XXXIII. *On the Electric and Magnetic Effects produced by the Motion of Electrified Bodies.* By J. J. THOMSON, *B.A., Fellow of Trinity College, Cambridge**.

§ 1. IN the interesting experiments recently made by Mr. Crookes (Phil. Trans. 1879, parts 1 and 2) and Dr. Goldstein (Phil. Mag. Sept. and Oct. 1880) on "Electric Discharges in High Vacua," particles of matter highly charged with electricity and moving with great velocities form a prominent feature in the phenomena; and a large portion of the investigations consists of experiments on the action of such particles on each other, and their behaviour when under the influence of a magnet. It seems therefore to be of some interest, both as a test of the theory and as a guide to future experiments, to take some theory of electrical action and find what, according to it, is the force existing between two moving electrified bodies, what is the magnetic force produced by such a moving body, and in what way the body is affected by a magnet. The following paper is an attempt to solve these problems, taking as the basis Maxwell's theory that variations in the electric displacement in a dielectric produce effects analogous to those produced by ordinary currents flowing through conductors.

For simplicity of calculation we shall suppose all the moving bodies to be spherical.

* Communicated by the Author.

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§ 2. The first case we shall consider is that of a charged sphere moving through an unlimited space filled with a medium of specific inductive capacity K.

The charged sphere will produce an electric displacement throughout the field; and as the sphere moves the magnitude of this displacement at any point will vary. Now, according to Maxwell's theory, a variation in the electric displacement produces the same effect as an electric current; and a field in which electric currents exist is a seat of energy; hence the motion of the charged sphere has developed energy, and consequently the charged sphere must experience a resistance as it moves through the dielectric. But as the theory of the variation of the electric displacement does not take into account any thing corresponding to resistance in conductors, there can be no dissipation of energy through the medium; hence the resistance cannot be analogous to an ordinary frictional resistance, but must correspond to the resistance theoretically experienced by a solid in moving through a perfect fluid. In other words, it must be equivalent to an increase in the mass of the charged moving sphere, which we now proceed to calculate.

Let a be the radius of the moving sphere, e the charge on the sphere, and let us suppose that the sphere is moving parallel to the axis of x with the velocity p; let ξ, η, ζ be the coordinates of the centre of the sphere; let f, g, h be the components of the electric displacement along the axes of x, y, z respectively at a point whose distance from the centre of the sphere is ρ, ρ being greater than a. Then, neglecting the self-induction of the system (since the electromotive forces it produces are small compared with those due to the direct action of the charged sphere), we have

$$f = -\frac{e}{4\pi}\frac{d}{dx}\frac{1}{\rho},$$

$$g = -\frac{e}{4\pi}\frac{d}{dy}\frac{1}{\rho},$$

$$h = -\frac{e}{4\pi}\frac{d}{dz}\frac{1}{\rho};$$

therefore

$$\frac{df}{dt} = -\frac{ep}{4\pi}\frac{d^2}{dx\,d\xi}\frac{1}{\rho},$$

$$\frac{dg}{dt} = -\frac{ep}{4\pi}\frac{d^2}{d\xi\,dy}\frac{1}{\rho},$$

$$\frac{dh}{dt} = -\frac{e}{4\pi}\frac{d^2}{d\xi\,dz}\frac{1}{\rho};$$

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hence

$$\left.\begin{aligned}\frac{df}{dt} &= \frac{ep}{4\pi}\frac{d^2}{dx^2}\frac{1}{\rho},\\ \frac{dg}{dt} &= \frac{ep}{4\pi}\frac{d^2}{dx\,dy}\frac{1}{\rho},\\ \frac{dh}{dt} &= \frac{ep}{4\pi}\frac{d^2}{dx\,dz}\frac{1}{\rho}.\end{aligned}\right\} \quad \ldots\ldots \quad (1)$$

Using Maxwell's notation, let F, G, H be the components of the vector-potential at any point; then, by 'Electricity and Magnetism,' § 616,

$$F = \mu\iiint\frac{u}{\rho'}\,dx\,dy\,dz,$$

$$G = \mu\iiint\frac{v}{\rho'}\,dx\,dy\,dz,$$

$$H = \mu\iiint\frac{w}{\rho'}\,dx\,dy\,dz,$$

where u, v, w are the components of the electric current through the element $dx\,dy\,dz$, and ρ' is the distance of that element from the point at which the values of F, G, H are required, μ is the coefficient of magnetic permeability. In the case under consideration,

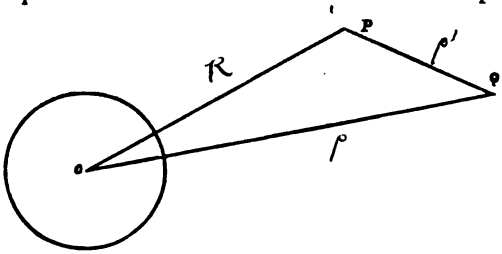
$$F = \mu\iiint\frac{\frac{df}{dt}}{\rho'}\,dx\,dy\,dz;$$

substituting for $\frac{df}{dt}$ its value from equation (1), we get

$$F = \frac{\mu ep}{4\pi}\iiint\frac{1}{\rho'}\frac{d^2}{dx^2}\frac{1}{\rho}\,dx\,dy\,dz,$$

with similar expressions for G and H.

Let us proceed to calculate the value of F at a point P.



Let O be the centre of the sphere; then $OQ=\rho$, $PQ=\rho'$, $OP=R$,

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$$F = \frac{\mu e p}{4\pi} \iiint \frac{1}{PQ} \frac{d^2}{dx^2} \frac{1}{\rho} dx\, dy\, dz.$$

Now $\frac{d^2}{dx^2} \frac{1}{\rho} = \frac{Y_2}{\rho^3}$, where Y_2 is a surface harmonic of the second order. And when $\rho > R$,

$$\frac{1}{PQ} = \frac{1}{\rho} + \frac{R}{\rho^2} Q_1 + \frac{R^2}{\rho^3} Q_2 + \ldots;$$

and when $\rho < R$,

$$\frac{1}{PQ} = \frac{1}{R} + \frac{\rho}{R^2} Q_1 + \frac{\rho^2}{R^3} Q_2 + \ldots;$$

where Q_1, Q_2, &c. are zonal harmonics of the first and second orders respectively referred to OP as axis.

Let Y'_2 denote the value of Y_2 along OP. Then, since $\int Y_n Q_m ds$, integrated over a sphere of unit radius, is zero when n and m are different, and $\frac{4\pi}{2n+1} Y'_n$ when $n=m$, Y'_n being the value of Y_n at the pole of Q_n, and since there is no electric displacement within the sphere,

$$F = \frac{\mu e p}{4\pi} \times \frac{4\pi Y'_2}{5} \left\{ \int_R^\infty \frac{R^2}{\rho^4} d\rho + \int_a^R \frac{\rho d\rho}{R^3} \right\}$$

$$= \frac{\mu e p}{5} Y'_2 \left(\frac{5}{6R} - \frac{a^2}{2R^3} \right),$$

or, as it is more convenient to write it,

$$= \frac{\mu e p}{5} \left(\frac{5R^2}{6} - \frac{a^2}{2} \right) \frac{d^2}{dx^2} \frac{1}{R}.$$

By symmetry, the corresponding values of G and H are

$$G = \frac{\mu e p}{5} \left(\frac{5R^2}{6} - \frac{a^2}{2} \right) \frac{d^2}{dx\, dy} \frac{1}{R},$$

$$H = \frac{\mu e p}{5} \left(\frac{5R^2}{6} - \frac{a^2}{2} \right) \frac{d^2}{dx\, dz} \frac{1}{R}.$$

These values, however, do not satisfy the condition

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0.$$

If, however, we add to F the term $\frac{2\mu e p}{3R}$, this condition will be satisfied; while, since the term satisfies Laplace's equation, the other conditions will not be affected: thus we have finally

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for points outside the sphere,

$$\left.\begin{aligned} F &= \frac{\mu e p}{5}\left(\frac{5R^2}{6} - \frac{a^2}{2}\right)\frac{d^2}{dx^2}\frac{1}{R} + \frac{2\mu e p}{3R}, \\ G &= \frac{\mu e p}{5}\left(\frac{5R^2}{6} - \frac{a^2}{2}\right)\frac{d^2}{dx\,dy}\frac{1}{R}, \\ H &= \frac{\mu e p}{5}\left(\frac{5R^2}{6} - \frac{a^2}{2}\right)\frac{d^2}{dx\,dz}\frac{1}{R}. \end{aligned}\right\} \quad . \quad . \quad (2)$$

Now, by 'Electricity and Magnetism,' § 634, T the kinetic energy

$$= \tfrac{1}{2}\iiint (Fu + Gv + Hw)\,dx\,dy\,dz,$$

in our case,

$$= \tfrac{1}{2}\iiint \left(F\frac{df}{dt} + G\frac{dg}{dt} + H\frac{dh}{dt}\right) dx\,dy\,dz.$$

Now

$$\tfrac{1}{2}\iiint F\frac{df}{dt}\,dx\,dy\,dz,$$

substituting for F and $\frac{df}{dt}$,

$$= \tfrac{1}{2}\frac{\mu e^2 p^2}{20\pi}\iiint \left(\frac{5r^2}{6} - \frac{a^2}{2}\right)\left(\frac{d^2}{dx^2}\frac{1}{r}\right)^2 dx\,dy\,dz,$$

since the term

$$\frac{\mu e^2 p^2}{12\,.\,\pi}\iiint \frac{1}{r}\frac{d^2}{dx^2}\frac{1}{r}\,dx\,dy\,dz$$

evidently vanishes.

Transforming to polars and taking the axis of x as the initial line, the above integral

$$= \frac{\mu e^2 p^2}{40\pi}\int_0^{2\pi}\int_0^{\pi}\int_a^{\infty} \left(\frac{5r^2}{6} - \frac{a^2}{2}\right)\frac{(3\cos^2\theta - 1)^2}{r^4}\sin\theta\,dr\,d\theta\,d\phi$$

$$= \frac{4\mu e^2 p^2}{75a},$$

$$\tfrac{1}{2}\iiint G\frac{dg}{dt}\,dx\,dy\,dz = \frac{\mu e^2 p^2}{40\pi}\iiint \left(\frac{5}{6}r^2 - \frac{a^2}{2}\right)\left(\frac{d^2}{dx\,dy}\frac{1}{r}\right)^2 dx\,dy\,dz.$$

By transforming to polars, as before, we may show that this

$$= \frac{\mu e^2 p^2}{25a}.$$

Similarly,

$$\tfrac{1}{2}\iiint H\frac{dh}{dt}\,dx\,dy\,dz = \frac{\mu e^2 p^2}{25a};$$

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$\therefore$ T, the kinetic energy due to the electrification

$$= \tfrac{1}{2}\iiint\left(\mathrm{F}\frac{df}{dt} + \mathrm{G}\frac{dg}{dt} + \mathrm{H}\frac{dh}{dt}\right) dx\,dy\,dz$$

$$= \frac{2\mu e^2 p^2}{15a}.$$

Hence, if m be the mass of the sphere, the whole kinetic energy

$$= \left(\frac{m}{2} + \frac{2}{15}\frac{\mu e^2}{a}\right) p^2; \quad \ldots\ldots \quad (3)$$

or the effect of the electrification is the same as if the mass of the sphere were increased by $\frac{4}{15}\frac{\mu e^2}{a}$, or, if V be the potential of the sphere, by $\frac{4}{15}\mu \mathrm{K}^2\mathrm{V}^2 a$.

To form some idea of what the increase of mass could amount to in the most favourable case, let us suppose the earth electrified to the highest potential possible without discharge, and calculate the consequent increase in mass. According to Dr. Macfarlane's experiments, published in the Philosophical Magazine for December 1880, the electric force in air at ordinary temperatures and pressures must not exceed 3×10^{12} (electromagnetic system of units). The electric force just outside the sphere is V/a; hence the greatest possible value of V is $3 \times 10^{12} a$, where a is the radius of the earth. Putting this value for V, $\mu = 1$, $\mathrm{K} = \frac{1}{9 \cdot 10^{20}}$, $a = 6{\cdot}4 \times 10^8$, we get for the corresponding value of the increase of mass 7×10^8 grms., or about 650 tons, a mass which is quite insignificant when compared with the mass of the earth.

For spheres of different sizes, the greatest increase in mass varies as the cube of the radius; hence the ratio of this increase to the whole mass of the sphere is constant for all spheres of the same material; for spheres of different materials the ratio varies inversely as the density of the material.

If the body moves so that its velocities parallel to the axes of x, y, z respectively are p, q, r, then it is evident that the effect of the electrification will be equivalent to an increase of $\frac{4}{15}\mu \mathrm{K}^2\mathrm{V}^2 a(p^2 + q^2 + r^2)$ in the mass of the sphere.

§ 3. To find the magnetic force produced by the moving sphere at any point in the field. By equations (2) we have, for points outside the sphere,

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$$F = \frac{\mu e p}{5}\left(\frac{5R^2}{6} - \frac{a^2}{2}\right)\frac{d^2}{dx^2}\frac{1}{R} + \frac{\mu e p}{3}\frac{2}{R},$$

$$G = \frac{\mu e p}{5}\left(\frac{5R^2}{6} - \frac{a^2}{2}\right)\frac{d^2}{dx\,dy}\frac{1}{R},$$

$$H = \frac{\mu e p}{5}\left(\frac{5R^2}{6} - \frac{a^2}{2}\right)\frac{d^2}{dx\,dr}\frac{1}{R}.$$

Now if α, β, γ be the components of the magnetic induction at the point (x, y, z),

$$\alpha = \frac{dH}{dy} - \frac{dG}{dz} = 0,$$

$$\beta = \frac{dF}{dz} - \frac{dH}{dx} = -\frac{\mu e p z}{R^4} = \mu e p \frac{d}{dz}\frac{1}{R},$$

$$\gamma = \frac{dG}{dx} - \frac{dF}{dy} = \frac{\mu e p y}{R^3} = -\mu e p \frac{d}{dy}\frac{1}{R}.$$

Hence we see, by symmetry, that if the sphere move with velocity q parallel to the axis of y, the corresponding values would be

$$\alpha = -\mu e q \frac{d}{dz}\frac{1}{R},$$

$$\beta = 0,$$

$$\gamma = \mu e q \frac{d}{dx}\frac{1}{R};$$

and if it moved with velocity r parallel to the axis of z, the corresponding values would be

$$\alpha = \mu e r \frac{d}{dy}\frac{1}{R},$$

$$\beta = -\mu e r \frac{d}{dx}\frac{1}{R},$$

$$\gamma = 0.$$

Hence, if p, q, r be the components of the velocity of the centre of the sphere parallel to the axes of x, y, z respectively, the components of magnetic induction are

$$\alpha = \mu e\left(r\frac{d}{dy}\frac{1}{R} - q\frac{d}{dz}\frac{1}{R}\right),$$

$$\beta = \mu e\left(p\frac{d}{dz}\frac{1}{R} - r\frac{d}{dx}\frac{1}{R}\right),$$

$$\gamma = \mu e\left(q\frac{d}{dx}\frac{1}{R} - p\frac{d}{dy}\frac{1}{R}\right);$$

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or they may also be written

$$\left.\begin{aligned} \alpha &= \frac{\mu e}{R^3}\big(q(z-\zeta)-r(y-\eta)\big), \\ \beta &= \frac{\mu e}{R^3}\big(r(x-\xi)-p(z-\zeta)\big), \\ \gamma &= \frac{\mu e}{R^3}\big(p(y-\eta)-q(x-\xi)\big). \end{aligned}\right\} \quad . \quad . \quad (4)$$

Comparing these expressions with those given by Ampère for the magnetic force produced by a current, we see that the magnetic force due to the moving sphere is the same as that produced per unit length of a current whose intensity is $\mu e\sqrt{p^2+q^2+r^2}$, situated at the centre of the sphere, the direction of the positive current coinciding with the direction of motion of the sphere. The resultant magnetic force produced by the sphere at any point is $\omega\mu e \sin\epsilon/\rho^2$, ω being the velocity of the sphere, and ϵ the angle between the direction of motion of the sphere and the radius vector ρ drawn from the centre of the sphere to the point; the direction of the force is perpendicular both to the direction of motion of the sphere and the radius vector from the centre of the sphere to the point; and the direction of the force and the direction of motion are related to each other like translation and rotation in a right-handed screw.

It may be useful to form a rough numerical idea of the magnitude of the greatest magnetic force which could be produced by a moving charged sphere. The greatest value of the force $=\mu KVa\omega/\rho^2$, where a is the radius and V the potential of the sphere. Now if F be the greatest electric force which can exist without discharge, the greatest value of V is Fa. According to Mr. Macfarlane's experiments F is, roughly speaking, about 3×10^{12}, $\mu K=\frac{1}{9\times10^{20}}$; substituting these values, the greatest value of the magnetic force becomes $\frac{1a^2\omega}{3\rho^2 10^8}$. Now $\frac{a}{\rho}$ cannot be greater than unity; so the greatest value of the force is $\omega/3\times10^8$. If the sphere were attached to an arm of such length that it described a metre in each complete revolution of the arm, and if the arm were to make 100 revolutions a second, ω would equal 10^4, and the greatest magnetic force would be $1/3\times10^4=\cdot000033$. Prof. Rowland, in his experiments on the magnetic effects of electric convection, measured a magnetic force only about one tenth of this.

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The result we have just obtained (viz. that a moving body charged with electricity produces the effect of an electric current) shows that Prof. Rowland's experiments on electric convection are in agreement with Maxwell's theory.

§ 4. The fact that a moving body charged with electricity produces a vector-potential in the field through which it is moving, suggests a possible theory of the cause of the green phosphorescence observed in vacuum-tubes at the places where the molecular streams strike the glass, different from that put forward by Mr. Crookes. It will be seen from the above work that the moving particle produces a vector-potential whose value depends on the velocity of the moving body. Now, when a particle strikes the glass directly, its velocity is reversed and the vector-potential changes sign; thus during the short time occupied by the collision the vector-potential must be changing very rapidly. But any change in the vector-potential produces a corresponding electromotive force, and thus the glass against which the molecules impinge is subjected to a rapidly varying electromotive force. But this, if Maxwell's electromagnetic theory of light be true, is exactly what it is subjected to when a beam of light falls upon it, which we know is the ordinary method of exciting phosphorescence. Stokes's law, that the period of the vibrations exciting the phosphorescence is smaller than the period of the emitted light, compels us to assume that at some period of the collision the velocity of the moving particle is changing at a greater rate than the rate of vibration of green light: in our present state of knowledge, however, there seems nothing impossible in this. This, too, would explain the following difficulty:—Since we have every reason for supposing the discharge in a vacuum-tube to be discontinuous, the vector-potential due to electricity moving through the tube will vary, producing a varying electromotive force all over the tube; another varying electromotive force will be produced by the action of the charge on the electrodes. Now it may be asked, why, if the above theory be true, does not this variable electromotive force make the whole tube phosphoresce, instead of the phosphorescence being confined to the places where the molecular streams strike the glass. But Spottiswoode and Moulton have proved (see Phil. Trans. for 1879, part 2) that the time occupied by the negative discharge is greater than the time occupied by the particles in going the length of the tube. Hence, even if we made the extravagant assumption that these molecules travel with a velocity as great as that of light, the time of discharge, and consequently the period of the electromotive force, would be greater than the period of vibration of light whose

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wave-length was the length of the tube, and so, by Stokes's law, could not produce a luminous phosphorescence.

It may be useful to form a rough estimate of the electromotive force which could be produced by a moving particle.

By equation (1) we see, if the particle be moving parallel to the axis of x with velocity p, that the greatest value of F at a point distant R from the centre of the particle is

$$\mu p\left(\frac{1}{\mathrm{R}}-\frac{a^2}{5\mathrm{R}^3}\right).$$

Now the greatest value of e, as before, is $\mathrm{K}\times 3\times 10^{12}\times a^2$,

$$\mu\mathrm{K}=\frac{1}{9\times 10^{20}};$$

hence the greatest value of F at the surface of the particle

$$=\frac{3\times 4\times 10^{12}pa}{5\times 9\times 10^{20}}.$$

Now during the collision let us represent p by $p_0\cos kr$, where $\frac{2\pi}{k}$ is less than the period of vibration of green light; R must be therefore at least 3×10^{15}; for a particle of air a is of the order 10^{-7}. Substituting, we get

$$\frac{d\mathrm{F}}{dt}=-\frac{4\times 10^5}{15\times 10^{20}}\mathrm{R}p_0\sin \mathrm{R}t;$$

or the maximum value of $\frac{d\mathrm{F}}{dt}$ is

$$\frac{4\times 3\times 10^{20}}{15\times 10^{20}}p^0=\tfrac{4}{5}p_0.$$

Now at present we know nothing about p_0; but it must be very much greater than the mean velocity of the air-molecules, which is about 5×10^4; if we substitute this value for it, we get the maximum value of $\frac{d\mathrm{F}}{dt}$ or the maximum electromotive force to be about 4×10^4, or about $\frac{1}{2500}$ of a volt per centimetre. Now, for sunlight the maximum electromotive force is about 6 volts per centimetre (Maxwell's 'Electricity and Magnetism,' § 793); and when we consider the immense number of particles which must be striking the glass at each instant, we have no difficulty in conceiving that the magnitude of the electromotive force due to the moving particle may be sufficient to cause phosphorescence. To show the rapidity with

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which these electromotive forces diminish with the distance, we will take the case of a particle stopped by a screen at a distance of $\frac{1}{100}$ of a millimetre from the glass, and compare the electromotive force at the glass with the electromotive force which would be produced at the glass if there were no screen. By substituting in the formula giving the electromotive force, we find that the electromotive force at the glass when the screen is present is only about $\frac{1}{10000}$ of what it is when the screen is away; and as the intensity of the phosphorescence will vary as the square of the electromotive force, we see that when the screen is present the phosphorescence is quite imperceptible. This explains an experiment of Goldstein's, in which he coated the glass with a layer of collodion whose thickness he estimated at a few hundredths of a millimetre, and found the glass behind quite black.

§ 5. To find the effect produced by a magnet on a moving electrified sphere. To do this we shall calculate the kinetic energy of the system; we can then, by means of Lagrange's equations, calculate the force on the sphere.

Let α, β, γ be the components of magnetic induction, α_1, β_1, γ_1 the components of magnetic force; if A, B, C be the components of magnetization,

$$\alpha=\alpha_1+4\pi A, \quad \beta=\beta_1+4\pi B, \quad \gamma=\gamma_1+4\pi C.$$

The kinetic energy of the system

$$=\frac{1}{8\pi}\iiint(\alpha\alpha_1+\beta\beta_1+\gamma\gamma_1)dx\,dy\,dz.$$

To get the force on the sphere due to the magnet, we only want that part of the kinetic energy which involves both the coordinates of the sphere and the coordinates of the magnet. We may write the kinetic energy as

$$\frac{1}{8\pi}\iiint(\alpha^2+\beta^2+\gamma^2-4\pi\alpha A-4\pi\beta B-4\pi\gamma C)dx\,dy\,dz.$$

Let F′, G′, H′ be the components of the vector-potential due to the magnet alone; then, by equation (4),

$$\left.\begin{aligned}\alpha&=\mu e\left(r\frac{d}{dy}\frac{1}{R}-q\frac{d}{dz}\frac{1}{R}\right)+\frac{dH'}{dy}-\frac{dG'}{dz},\\ \beta&=\mu e\left(p\frac{d}{dz}\frac{1}{R}-r\frac{d}{dx}\frac{1}{R}\right)+\frac{dF'}{dz}-\frac{dH'}{dx},\\ \gamma&=\mu e\left(q\frac{d}{dx}\frac{1}{R}-p\frac{d}{dy}\frac{1}{R}\right)+\frac{dG'}{dx}-\frac{dF'}{dy}.\end{aligned}\right\}$$

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The part of the kinetic energy we are concerned with will evidently be

$$\frac{1}{8\pi}\iiint 2\mu e\Big[\Big(r\frac{d}{dy}-q\frac{d}{dz}\Big)\frac{1}{\mathrm{R}}\Big(\frac{d\mathrm{H}'}{dy}-\frac{d\mathrm{G}'}{dz}\Big)+\Big(p\frac{d}{dz}-r\frac{d}{dx}\Big)\frac{1}{\mathrm{R}}\Big(\frac{d\mathrm{F}'}{dz}-\frac{d\mathrm{H}'}{dx}\Big)+\Big(q\frac{d}{dx}-p\frac{d}{dy}\Big)\frac{1}{\mathrm{R}}\Big(\frac{d\mathrm{G}'}{dx}-\frac{d\mathrm{F}'}{dy}\Big)\Big]\,dx\,dy\,dz$$

$$-\frac{1}{8\pi}\iiint\mu e4\pi\Big[\mathrm{A}\Big(r\frac{d}{dy}-q\frac{d}{dz}\Big)\frac{1}{\mathrm{R}}+\mathrm{B}\Big(p\frac{d}{dz}-r\frac{d}{dx}\Big)\frac{1}{\mathrm{R}}+\mathrm{C}\Big(q\frac{d}{dx}-p\frac{d}{dy}\Big)\frac{1}{\mathrm{R}}\Big]\,dx\,dy\,dz.$$

Let us take the first integral first, and take the term depending on p; this is

$$\frac{\mu ep}{4\pi}\iiint\frac{d}{dz}\frac{1}{\mathrm{R}}\Big(\frac{d\mathrm{F}'}{dz}-\frac{d\mathrm{H}'}{dx}\Big)-\frac{d}{dy}\frac{1}{\mathrm{R}}\Big(\frac{d\mathrm{G}'}{dx}-\frac{d\mathrm{F}'}{dy}\Big)\,dx\,dy\,dz.$$

Integrating by parts this becomes

$$-\frac{\mu ep}{4\pi}\iint\mathrm{F}'\Big(\frac{d}{dx}\frac{1}{\mathrm{R}}\,dy\,dz+\frac{d}{dy}\frac{1}{\mathrm{R}}\,dx\,dz+\frac{d}{dz}\frac{1}{\mathrm{R}}\,dx\,dy\Big)$$

$$+\frac{\mu ep}{4\pi}\iint\frac{1}{\mathrm{R}}\Big(\frac{d\mathrm{H}'}{dx}\,dy\,dx+\frac{d\mathrm{G}'}{dx}\,dx\,dz+\frac{d\mathrm{F}'}{dx}dz\,dy\Big)$$

$$+\frac{\mu ep}{4\pi}\iiint\frac{1}{\mathrm{R}}\frac{d}{dx}\Big(\frac{d\mathrm{F}'}{dx}+\frac{d\mathrm{G}'}{dy}+\frac{d\mathrm{H}'}{dz}\Big)$$

$$-\mathrm{F}'\Big(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\Big)\frac{1}{\mathrm{R}}\,dx\,dy\,dz.$$

The surface-integrals are to be taken over the surface of the sphere; and the triple integral is to be taken throughout all space exterior to the sphere.

If the sphere be so small that we may substitute for the values of F', $\frac{d\mathrm{F}'}{dx}$, &c. at the surface their values at the centre of the sphere, the first surface-integral $=\mu ep\mathrm{F}'_1$, where F'_1 is the value of F' at the centre of the sphere; the second surface-integral vanishes, and the triple integral also vanishes, since

$$\frac{d^2}{dx^2}\frac{1}{\mathrm{R}}+\frac{d^2}{dy^2}\frac{1}{\mathrm{R}}+\frac{d^2}{dz^2}\frac{1}{\mathrm{R}}=0,$$

and

$$\frac{d\mathrm{F}'}{dx}+\frac{d\mathrm{G}'}{dy}+\frac{d\mathrm{H}'}{dz}=0.$$

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The part depending on p in the second integral

$$= -\frac{\mu e p}{2}\iiint\left(\mathrm{B}\frac{d}{dz}\frac{1}{\mathrm{R}} - \frac{\mathrm{C}d}{dy}\frac{1}{\mathrm{R}}\right)dx\,dy\,dz,$$

or (see Maxwell's 'Electricity and Magnetism,' § 405)

$$= -\frac{\mu e p}{2}\mathrm{F'}_1.$$

Adding this to the term $\mu e p \mathrm{F'}_1$ already obtained, we get $\frac{\mu e p}{2}\mathrm{F'}_1$ as the part of the kinetic energy depending on p. We have evidently similar expressions for the parts of the kinetic energy depending on q and r. Hence the part of the kinetic energy with which we are concerned will

$$= \frac{\mu e}{2}\cdot(\mathrm{F'}_1 p + \mathrm{G'}_1 q + \mathrm{H'}_1 r).$$

By Lagrange's equations, the force on the sphere parallel to the axis of x

$$= \frac{d\mathrm{T}}{dx} - \frac{d}{dt}\frac{d\mathrm{T}}{d\dot{x}}$$

$$= \frac{\mu e}{2}\left\{p\frac{d\mathrm{F'}_1}{dx} + q\frac{d\mathrm{G'}_1}{dx} + r\frac{d\mathrm{H'}_1}{dx} - \frac{d\mathrm{F'}_1}{dt}\right\}$$

$$= \frac{\mu e}{2}\left(p\frac{d\mathrm{F'}_1}{dx} + q\frac{d\mathrm{G'}_1}{dx} + r\frac{d\mathrm{H'}_1}{dx} - p\frac{d\mathrm{F'}_1}{dx} - q\frac{d\mathrm{F'}_1}{dy} - r\frac{d\mathrm{F'}_1}{dz}\right)\right\}$$

$$= \frac{\mu e}{2}\left\{q\left(\frac{d\mathrm{G'}_1}{dx} - \frac{d\mathrm{F'}_1}{dy}\right) - r\left(\frac{d\mathrm{F'}_1}{dz} - \frac{d\mathrm{H'}_1}{dx}\right)\right\}$$

$$= \frac{\mu e}{2}(qc_1 - rb_1).$$

Similarly, the force parallel to the axis of y

$$= \frac{\mu e}{2}(ra_1 - pc_1), \quad \ldots\ldots \quad (5)$$

the force parallel to the axis of z

$$= \frac{\mu e}{2}(pb_1 - qa_1),$$

where a_1, b_1, c_1 are the components of magnetic induction at the centre of the sphere due to the external magnet. These forces are the same as would act on unit length of a conductor at the centre of the sphere carrying a current whose components are $\frac{\mu e p}{2}$, $\frac{\mu e q}{2}$, $\frac{\mu e r}{2}$. The resultant force is perpendi-

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cular to the direction of motion of the sphere and to the magnetic induction; and if ω be the resultant velocity of the sphere, and θ the angle between the direction of motion of the sphere and the direction of magnetic induction, the magnitude of the force

$$= \frac{\mu e}{2} \omega \sqrt{a^2 + b^2 + c^2} \sin \theta.$$

It will be useful to endeavour to calculate the magnitude of this force on a particle of air moving in a vacuum-tube; although our knowledge of the magnitude of several of the quantities involved is so vague that our result must only be looked upon as showing that the force is of an order great enough to produce appreciable effects, and must not be looked upon as having any quantitative value.

Let us suppose that the mass of a molecule of air is 10^{-22} (C.G.S. system); that a, the radius of the molecule, $= 10^{-7}$; that, as before, $e = K \times 3 \times 10^{12} a^2 = K \times 3 \times 10^{-3}$ (this quantity is probably enormously underrated); and as we know nothing about the velocity of the charged particles, let us assume it to be the mean velocity of the air-molecules, viz. 4×10^{-5}. We shall suppose the vacuum-tube placed in a magnetic field whose strength is 10^3. Then, by the formula, the acceleration of the particle of air when the magnetic force is at right angles to its path is about 10^7; this acceleration would produce a deflection of about 2 millims. per decimetre of path, a deflection which could easily be observed. We know from the experiments of Mr. Crookes and others that a magnet produces very decided deflections of the molecular streams; and the direction of the deflections (see Phil. Trans. 1879, part 1, pp. 154 & 156) agrees with that given by formulæ (5), if we suppose that the particles projected from the negative pole are negatively charged.

§ 6. Let us now calculate the expression given by Maxwell's theory for the force between two charged moving particles.

Let u, v, w be the components of the velocity of the centre of one of the particles, u', v', w' those of the other; let R denote the distance between the particles, e the charge on one of the particles, e' the charge on the other; let r denote the distance of a point from the centre of the first particle, r' the distance of the same point from the centre of the second particle. We shall suppose, for the sake of simplicity, that the particles are very small; we shall calculate the kinetic energy of the system and deduce the forces between the particles by means of Lagrange's equations.

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The kinetic energy

$$= \tfrac{1}{2}\iiint\left(\mathrm{F}\frac{df}{dt}+\mathrm{G}\frac{dg}{dt}+\mathrm{H}\frac{dh}{dt}\right)dx\,dy\,dz.$$

Now

$$\mathrm{F}=\frac{\mu}{5}\Big[e\Big(u\frac{d^2}{dx^2}\frac{1}{\mathrm{R}}+v\frac{d^2}{dx\,dy}\frac{1}{r}+w\frac{d^2}{dx\,dz}\frac{1}{r}\Big)\Big(\frac{5r^2}{6}-\frac{a^2}{2}\Big)+e\frac{10}{3}\frac{u}{r}$$

$$+e'\Big(u'\frac{d^2}{dx^2}\frac{1}{r'}+v'\frac{d^2}{dx\,dy}\frac{1}{r'}+w'\frac{d^2}{dx\,dz}\frac{1}{r'}\Big)\Big(\frac{5r'^2}{6}-\frac{a'^2}{2}\Big)+\frac{e'10}{3}\frac{u'}{r'}\Big],$$

with similar expressions for G and H.

$$\frac{df}{dt}=\frac{1}{4\pi}\Big[e\Big(u\frac{d^2}{dx^2}\frac{1}{r}+v\frac{d^2}{dx\,dy}\frac{1}{r}+w\frac{d^2}{dx\,dz}\frac{1}{r}\Big)$$

$$+e'\Big(u'\frac{d^2}{dx^2}\frac{1}{r'}+v'\frac{d^2}{dx\,dy}\frac{1}{r'}+w'\frac{d^2}{dx\,dz}\frac{1}{r'}\Big)\Big],$$

with similar expressions for $\frac{dg}{dt}$ and $\frac{dh}{dt}$. Since the particles are supposed to be very small, we shall neglect those terms in F which depend on a^2 and a'^2.

The part of the kinetic energy we are concerned with involves the product $e\,e'$: let us first calculate that part of it arising from the product of that part of F due to e with that part of $\frac{df}{dt}$ due to e'. We shall take the line joining the particle as the axis of x; and for brevity we shall denote $\frac{\mu e e'}{24\pi}$ by σ.

The coefficient of $u\,u'$ in the part of the kinetic energy we are considering

$$=\sigma\iiint\Big(\frac{d^2}{dx^2}\frac{1}{r}+\frac{4}{r^3}\Big)r^2\frac{d^2}{dx^2}\frac{1}{r'}\,dx\,dy\,dz.$$

Now, for values of $r>\mathrm{R}$,

$$\frac{1}{r'}=\frac{1}{r}-\mathrm{R}\frac{d}{dx}\frac{1}{r}+\frac{\mathrm{R}^2}{2!}\frac{d^2}{dx^2}\frac{1}{r}-\ldots;$$

$$\therefore\ \frac{d^2}{dx^2}\cdot\frac{1}{r'}=\frac{d^2}{dx^2}\frac{1}{r}-\mathrm{R}\frac{d^3}{dx^3}\frac{1}{r}+\ldots.$$

Now, since

$$\frac{d^n}{dx^n}\frac{1}{r}=(-)^n\frac{n!}{r^{n+1}}\mathrm{Q}_n,$$

where Q_n is a zonal harmonic of the nth order; and since the product of two harmonics of different degrees integrated over

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the surface of a sphere vanishes, we may substitute in the integral $\frac{d^2}{dx^2}\frac{1}{r}$ for $\frac{d^2}{dx^2}\frac{1}{r'}$; then, transforming to polars, the integral

$$= \sigma \int_0^{2\pi} \int_0^{\pi} \int_R^{\infty} 4Q_2^2 \frac{1}{r^3} \sin\theta \, d\phi \, dv \, dr$$

$$= \frac{16\pi\sigma}{5R};$$

for values of $r < R$,

$$\frac{1}{r'} = \frac{1}{R} + \frac{rQ_1}{R^2} + \frac{rQ_2}{R^3} + \ldots.$$

Now $r^n Q_n$ is a solid harmonic of the nth order; hence $\frac{d^2}{dx^2}(r^n Q_n)$ is a solid harmonic of the $(n-2)$th order; and in particular $\frac{d^2}{dx^2}(r^4 Q_4)$ is a solid harmonic of the second order; and, by the same reasoning as before, we may substitute in the integral $\frac{1}{R^5}\frac{d^2}{dx^2}(r^4 Q_4)$ for $\frac{d^2}{dx^2}\frac{1}{r'}$. Now

$$r^4 Q_4 = \frac{35x^4 - 30x^2(x^2+y^2+z^2) + 3(x^2+y^2+z^2)^2}{8};$$

$$\therefore \frac{d^2}{dx^2}(r^4 Q_4) = 12x^2 - 6(y^2+z^2) = 12r^2 Q_2.$$

So for values of $r < R$ the integral becomes

$$\frac{\sigma}{R^5} \int_0^{2\pi} \int_0^{\pi} \int_0^{R} 24 Q_2^2 r^3 \sin\theta \, d\phi \, d\theta \, dr$$

$$= \frac{24\sigma\pi}{5R}.$$

Adding this to the part of the integral for $r > R$, we get for the coefficient of uu', $\frac{8\pi\sigma}{R}$. The coefficients of uv' and uw' vanish by inspection.

The coefficient of vv'

$$= \sigma \iiint r^2 \frac{d^2}{dx\,dy}\frac{1}{r}\,\frac{d^2}{dx\,d}\frac{1}{r'}\,dx\,dy\,dz.$$

Now when $r > R$ we may, by the same reasoning as before, substitute $\frac{d^2}{dx\,dy}\frac{1}{r}$ for $\frac{d^2}{dx\,dy}\frac{1}{r'}$ in the integral, and it becomes

$$\sigma \iiint \frac{9r^2x^2y^2}{r^{10}}\,dx\,dy\,dz,$$

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or transforming to polars,

$$9\sigma\int_0^{2\pi}\int_0^{\pi}\int_R^{\infty}\frac{\sin^2\theta\cos^2\theta\cos^2\phi}{r^2}\sin\theta\,d\phi\,d\theta\,dr$$
$$=\frac{36\pi\sigma}{15R}.$$

For values of $r<R$ we may, by the same reasoning as before, substitute $\frac{1}{R^5}\frac{d^2}{dx\,dy}(r^4Q_4)$ in the integral for $\frac{d^2}{dx\,dy}\frac{1}{r'}$. Now

$$\frac{d^2}{dx\,dy}(r^4Q_4)=-12xy;$$

making this substitution, the integral becomes

$$-\frac{\sigma}{R^5}\iiint\frac{36x^2y^2}{r^5}dx\,dy\,dz$$
$$=-\frac{\sigma}{R^5}\int_0^{2\pi}\int_0^{\pi}\int_0^{R}36r^3\sin^3\theta\cos^2\theta\cos^2\phi\,d\phi\,d\theta\,dr$$
$$=-\frac{36\pi\sigma}{15R}.$$

Hence, adding this to the part previously obtained for values of $r>R$, we see that the coefficient of vv' from $F\frac{df}{dt}$ is zero, and, similarly, the coefficient of ww' from this part of the integral vanishes.

Let us now take the terms arising from $\iiint G\frac{dg}{dt}\,dx\,dy\,dz$, and take, as before, the part arising from the product of that part of G due to e with the part of $\frac{dg}{dt}$ due to e'. The coefficient of uu' in this part will be the same as the coefficient of vv' in the former part, and so will vanish.

The coefficient of vv'

$$=\sigma\iiint\left(r^2\frac{d^2}{dy^2}\frac{1}{r}+\frac{4}{r}\right)\frac{d^2}{dx^2}\frac{1}{r'}dx\,dy\,dz.$$

Now for values of $r>R$ we may, as before, substitute $\frac{d^2}{dy^2}\frac{1}{r}$ for $\frac{d^2}{dy^2}\frac{1}{r'}$; and it becomes

$$\sigma\iiint r^2\left(\frac{3y^2-r}{r}\right)^2dx\,dy\,dz.$$

By transforming to polars, as before, this may be shown to be

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$\frac{16\sigma\pi}{5}$. For values of $r<R$ we may, as before, substitute $\frac{1}{R^5}\frac{d^2}{dy^2}(r^4Q_4)$ for $\frac{d^2}{dy^2}\frac{1}{r'}$ in the integral. Now

$$\frac{d^2}{dy^2}(r^4Q_4)=\frac{36y^2+12z^2-48x^2}{8};$$

$\therefore$ the integral

$$=\frac{\sigma}{R^5}\iiint\frac{r^2(3y^2-r^2)(36y^2+12z^2-48x^2)}{8r^5}\,dx\,dy\,dz.$$

By transforming to polars, this may be shown to be $\frac{9\pi\sigma}{5R}$. Adding this to the part of the integral due to values of $r>R$, we get for the coefficient of vv',

$$\frac{5\sigma\pi}{R}.$$

As before, the coefficients of uv', vu', uw', &c. disappear by inspection.

The coefficient of ww'

$$=\sigma\iiint r^2\frac{d^2}{dy\,dz}\frac{1}{r}\frac{d^2}{dy\,dz}\frac{1}{r'}\,dx\,dy\,dz;$$

substituting, for values of $r>R$, as before $\frac{d^2}{dy\,dz}\frac{1}{r}$ for $\frac{d^2}{dy\,dz}\frac{1}{r'}$ in the integral, it becomes

$$\sigma\iiint\frac{9y^2z^2}{r^8}\,dx\,dy\,dz,$$

which, by transforming to polars, may be shown to be $\frac{12\sigma\pi}{5R}$. For values of $r<R$ we may, as before, substitute $\frac{1}{R^5}\frac{d^2}{dy\,dz}(r^4Q_4)$ for $\frac{d^2}{dy\,dz}\frac{1}{r'}$ in the integral. Now

$$\frac{d^2}{dy\,dz}(r^4Q_4)=3yz.$$

On making this substitution, the integral

$$=\frac{\sigma}{R^5}\iiint\frac{9y^2z^2}{r^3}\,dx\,dy\,dz=\frac{3\sigma\pi}{5R}.$$

Adding this to the part obtained before, we get for the coefficient of ww',

$$\frac{12\sigma\pi}{5R}+\frac{3\sigma\pi}{5R},\text{ or }3\sigma\pi.$$

From the part of $\iiint H\frac{dh}{dt}\,dx\,dy\,dz$ which arises from that part of H due to e and that part of $\frac{dh}{dt}$ due to e', we can see,

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by the preceding work, that the coefficient of uu' is zero; the coefficient of vv', $3\sigma\pi$; and the coefficient of ww', $5\sigma\pi$. Adding, we get the whole kinetic energy due to the vector-potential arising from e and the electric displacement arising from e'

$$= \frac{\pi\sigma}{2R}\left(8uu' + (5+3)vv' + (5+3)ww'\right)$$

$$= \frac{4\pi\sigma}{R}(uu' + vv' + ww').$$

We can get that part of the kinetic energy due to the vector-potential arising from e' and the electric displacement from e by writing e' for e, and u', v', w' for u, v, w respectively. Hence, that part of the kinetic energy which is multiplied by ee'

$$= \frac{8\pi\sigma}{R}(uu' + vv' + ww');$$

or, substituting for σ its value,

$$= \frac{\mu ee'}{3R}(uu' + vv' + ww').$$

Or if q and q' be the velocities of the spheres, and ϵ the angle between their directions of motion, this part of the kinetic energy

$$= \frac{\mu ee'}{3R} qq' \cos\epsilon,$$

and the whole kinetic energy due to the electrification

$$= \mu\left(\frac{2}{15}\frac{e^2q^2}{a} + \frac{2}{15}\frac{e'^2q'^2}{a_1} + \frac{ee'}{3R} qq' \cos\epsilon\right). \quad . \quad . \quad (6)$$

If x, y, z be the coordinates of the centre of one sphere, x', y', z' those of the other, we may write the last part of the kinetic energy in the form

$$\frac{\mu ee'}{3R}\left(\frac{dx}{dt}\frac{dx'}{dt} + \frac{dy}{dt}\frac{dy'}{dt} + \frac{dz}{dt}\frac{dz'}{dt}\right).$$

By Lagrange's equations, the force parallel to the axis of x acting on the first sphere

$$= \frac{dT}{dx} - \frac{d}{dt}\left(\frac{dT}{d\cdot\frac{dx}{dt}}\right)$$

$$= \frac{\mu ee'}{3}\left\{\left(\frac{dx}{dt}\frac{dx'}{dt} + \frac{dy}{dt}\frac{dy'}{dt} + \frac{dz}{dt}\frac{dz'}{dt}\right)\frac{d}{dx}\frac{1}{R} - \frac{d}{dt}\left(\frac{\frac{dx'}{dt}}{R}\right)\right\},$$

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with similar expressions for the components of the force parallel to the axes of y and z.

From this we see that if $\dot{q}_1$ be the acceleration of the second sphere, the forces on the first sphere are an attraction $\frac{\mu ee'}{3R^2} qq' \cos e$ along the line joining the centres of the spheres, a force $\frac{\mu ee'}{3R} \dot{q}_1$ in the direction opposite to the acceleration of the second sphere, and a force $\frac{\mu ee'}{3} q_1 \frac{d}{dt}\left(\frac{1}{R}\right)$ in the direction opposite to the direction of motion of the second sphere. There are, of course, corresponding forces on the second sphere; and we see that, unless both spheres move with equal uniform velocities in the same direction, the forces on the two spheres are not equal and opposite. If we suppose that the two spheres are moving with uniform velocities q in the same direction, the repulsion between them is $\frac{ee'}{K.R^2}\left(1-\frac{\mu K q^2}{3}\right)$; or if c be the velocity of light in the medium through which they are moving, the repulsion $= \frac{ee'}{KR^2}\left(1-\frac{q^2}{3c^2}\right)$. Hence, if the repulsion between two electrified particles is to be changed into an attraction by means of their motion, their velocities must exceed $\sqrt{3}c$; hence we should expect the molecular streams in a vacuum-tube to repel each other, as we could not suppose that the velocity of the particles forming these streams is as great as that of light; and Mr. Crookes has, in fact (see Phil. Trans. 1879, part ii.), experimentally determined that they do repel each other.

It is remarkable that the law of force between two moving charged particles, which we have deduced from Maxwell's theory, agrees with that assumed by Clausius, in his recent researches on Electrodynamics (see Phil. Mag. Oct. 1880); but it differs from Weber's well-known law materially. According to Weber's law, the force does not depend on the actual velocities of the particles, but only on their velocity relative to each other, whereas, according to the laws we have investigated, the forces depend on the actual velocities of the particles as well as on their relative velocities: thus there is a force between two charged particles moving with equal velocities in the same direction, in which case, of course, the relative velocity is nothing. It must be remarked that what we have for convenience called the actual velocity of the particle is, in fact, the velocity of the particle relative to the medium through which it is moving: thus, in equation (6), q, q' are the velocities of the first and second particles respectively relative to the medium whose magnetic permeability is μ.

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Clausius, in the paper previously referred to, explains the various phenomena produced by currents by means of this law of force, and the hypothesis that a current consists of streams of opposite electricities moving in opposite directions. Now, since the expressions we have obtained for the force between the particle do not depend on the specific inductive capacity of the medium, but only on its magnetic permeability, if we make this assumption about the nature of a current, it follows from Maxwell's theory that the electrodynamic phenomena produced by a current of given strength do not depend on the specific inductive capacity of the surrounding medium, though they do depend on its magnetic permeability.

Faraday, in his 'Experimental Researches' (§ 1709 and onwards), describes some experiments which he made to determine whether altering the surrounding medium produced any change in the electromagnetic action of a current. The result of the experiments was that he was unable to detect any such change; but in his experiments, though the specific inductive capacities of the various media tried were very different, their magnetic permeabilities were all of them very nearly unity.

XXXIV. *Theoretical Explanations of the Rectilinear Transmission and Spontaneous Diffusion of Sound and Light.* *By* Professor Challis, M.A., F.R.S., F.R.A.S.*

When any disturbance is produced at a given position A in an unlimited mass of elastic fluid of perfect fluidity, defined by the relation $p=a^2\rho$ between its pressure p and density ρ, a^2 being constant, it is found by experience that there will be a resulting state of the fluid at a point P, whose position is taken *ad libitum*, and at all intermediate points between A and P. In other words, there will be a rectilinear transmission of effect from A to all surrounding points, without *respect to the particular mode of disturbing the fluid.* It may be that a difference of effect in different directions may depend on the mode of disturbance; at the same time it is found that a resulting disturbance is produced at *all* points, whatever be the mode of disturbance. In treatises on hydrodynamics this remarkable fact is left out of consideration. I know of none in which this problem has been solved, or even proposed. But it is an admitted principle that when the *fundamentals* of any branch of applied science, after being established by observation and experiment, have been ex-

* Communicated by the Author.

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pressed by general equations, all other related facts of observation admit of being accounted for by mathematical reasoning derived from the equations. The science of Analytical Hydrodynamics must be pronounced to be in a very imperfect state, unless it be capable of giving a reason for such a patent and general fact as that just mentioned. The argument I am about to adduce is intended to give the required explanation. It will be proper to premise here that this argument has nothing to do with the necessity I have so often insisted upon, of a third general equation for completing the analytical principles of hydrodynamics. I still maintain that the principles on which that equation rests, and the process of its deduction, are so simple and direct that they do not admit of being controverted; but they do not come under consideration in the present enquiry.

Supposing the reasoning to apply to a portion of the fluid contained in a straight rectangular tube of small uniform transverse section extending from A to P, it is evident that if arbitrary motion be communicated to the fluid by a movable diaphragm at A exactly fitting the tube, rectilinear motion will take place in the direction of the axis of the tube. This problem has been discussed by Poisson in the *Journal de l'Ecole Polytechnique*, tom. vii., for the case in which $p=a^2\rho$; and from the mathematical investigation he deduces a particular integral from which he infers (p. 369) that the original disturbance will be transmitted uniformly with a velocity equal to a. The same problem has been solved by Mr. Earnshaw, in the Philosophical Transactions for 1860, p. 133, by a process differing only in form from that of Poisson, and conducting to the same results. From this integral I obtained in a particular case the absurd conclusion that the same particle of fluid might be at rest and have a maximum motion at the same instant of time (see Philosophical Magazine for June 1848, p. 496, and my 'Principles of Mathematics and Physics,' p. 195). The reasoning by which this result was reached is so certain that it does not admit of being questioned, and, in fact, has not been directly questioned; but Mr. Earnshaw has thought good to say, that the wave "will force its way in violation of our equations." This is so strange and inadmissible an assertion that I have a right to regard it as a virtual acknowledgment of the *reductio ad absurdum* above mentioned, the reality of which I shall accordingly take for granted.

But a *reductio ad absurdum* is not a result which can be slurred over. It is absolutely necessary for the satisfactory establishment of the principles of any science in which it

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occurs, that the absurdity should be accounted for and got rid of. I have adverted above to *two* modes in which a disturbance at A may be rectilinearly transmitted from A, so as to have effect at any point P. In the first it was stated that, as *matter of experience*, the effect takes place independently of any particular conditions under which the fluid was disturbed at A; in the other the fluid is assumed to be disturbed in a particular manner, and under certain conditions. The former of these modes may be called, for distinction, *free* or *spontaneous* motion, and the other constrained or arbitrary motion. Now, in the received analytical theory of hydrodynamics, the determination of the rate of propagation is made to depend on the conditions of the arbitrary mode of disturbance, whereas, as matter of fact, the propagation and rate of propagation are both independent of the particular form of disturbance. This contradiction of the theory by experimental facts sufficiently accounts for the *reductio ad absurdum*, which, consequently, signifies that the theoretical reasoning is at fault and requires to be rectified. To show how this is to be done is the purpose of the following investigation.

The problem to be solved is to account for the fact of a rectilinear transmission of effect from a disturbance made at a certain point A to *any* point P in the surrounding fluid, without reference to any assigned mode of disturbing the fluid. Since there are no arbitrary initial conditions of the motion, it follows that the kind of motion must be indicated by some general analytical circumstance. It will here be supposed that the required analytical circumstance is that the differential function $udx+vdy+wdz$ is an exact differential in all cases. On this supposition it may be assumed that there will be a rectilinear *axis* of motion between A and P, the motion relative to the axis satisfying the condition of the integrability of $udx+vdy+wdz$. Also in order that there may be motion persistently along the axis, it is necessary that the condensation pertaining to the motion should be symmetrically disposed about the axis, so that both the condensation and consequent transverse velocity will be functions of the distance (r) from the axis. Consequently, supposing that the axis of z coincides with the given axis of motion, we have to assume that, with reference to that axis,

$$(d.f\phi)=udx+vdy+wdz,$$

f being a function of r, and ϕ a function of z and f. Accordingly,

$$u=\phi\frac{df}{dx},\quad v=\phi\frac{df}{dy},\quad w=f\frac{d\phi}{dz}.$$

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Since f is a function of r—that is, of $(x^2+y^2)^{\frac{1}{2}}$—we have $\frac{df}{dx}=\frac{x}{r}$, $\frac{df}{dy}=\frac{y}{r}$; and hence, for any point on the axis, $u=0$ and $v=0$, as is required by the foregoing supposition. Also, because for every point on the axis $\frac{df}{dx}=0$, and $\frac{df}{dy}=0$, it follows that f has at each point a maximum or minimum value, which, in the absence of determining conditions, we may assume to be unity. Hence the velocity w along the axis will be expressed by $\frac{d\phi}{dz}$.

From these results, taken in conjunction with the first and second general hydrodynamical equations, we might proceed to obtain actual values, expressed in series, of the quantities f and ϕ, from which the motions along and parallel to the axis, and in the directions transverse to the axis, might be inferred. It is not necessary to introduce here these investigations, since I have already given them at length in arts. 20–28 of a communication, "on the Mathematical Theory of the Vibrations of an Elastic Fluid," inserted in the Philosophical Magazine for August 1862, and also in my work on the Principles of Mathematics and Physics, pp. 201–211. It is, at present, chiefly of importance to remark that, as I have been careful to point out, all this investigation is *prior* to the supposition of any arbitrary method of putting the fluid in motion, and consequently has exclusive reference to laws of the mutual action of the parts of the fluid irrespective of arbitrary conditions; or, as I have said in the present communication, the motion is free, and spontaneous as to its laws.

Proceeding now to cases of arbitrary disturbance, it is first to be stated that, as the circumstances of spontaneous motion were determined prior to any discussion of arbitrarily imposed motion, the treatment of the latter must take into account the laws obtained relative to spontaneous motion. Accordingly the result of any arbitrary disturbance must be supposed to be a *composition* of spontaneous motions unlimited as to number and directions, and originating at the place of disturbance. The component motions may be designated as *elementary* motions, which, at the same time that they satisfy by their composition the given conditions of the disturbance, severally obey the laws of spontaneous motion obtained as above stated. They are capable of this application on account of their being derived from linear differential equations with constant coefficients. Since the position of the point P was taken *ad libitum*, what has been argued relative to the resulting

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motion at P applies to every portion of the fluid surrounding A. Here, then, is the explanation of that *diffusion* of the effect of any disturbance which it was proposed to account for.

The *rate of transmission* of the effect of the disturbance has to be calculated from the laws of the *elementary* motions, without respect to any arbitrary mode of disturbance. This I have done by the solution of Proposition XIV. in pages 214–224 of my treatise on the Principles of Mathematics and Physics, obtaining thereby the result that the theoretical value exceeds the observed value by 17·5 feet. The excess may with probability be ascribed to the circumstance that the air is not, as supposed in the theory, a simple fluid of perfect elasticity, but composite and loaded with vapour.

It would appear from the foregoing argument that the method of determining velocity of propagation in an elastic fluid from assumed conditions of the motion of the fluid, which was first adopted by Newton, and afterwards virtually followed by Lagrange and Poisson, is defective in principle, and that the exact method must be derived from the general equations of fluid-motion. Also it seems that the rapid alternate generations of heat and cold in a series of aerial vibrations does not, as has been generally supposed, sensibly affect the rate of propagation.

The results above obtained relative to the diffusion of sound by aerial vibrations equally pertain to the diffusion of light by ætherial vibrations, on the hypothesis, which I adopt, that the æther is a perfect fluid defined by the equation $p=a^2\rho$, a^2 being absolutely constant.

The motion of the air defined by the series for the function ϕ, obtained in the manner already stated, is required for ascertaining the character of spontaneous aerial vibrations pertaining to the phenomena of sound, and for obtaining the laws of the harmonic series which is applied in the theory of *music*.

The motion of the æther defined by the spontaneous series for f, obtained as before stated, together with that for ϕ, is necessary for ascertaining the character of a *ray of light*, the laws of transverse vibrations and of polarization, and the mode of derivation of a polarized ray from a ray of common light.

I think it right to mention here that the assertion that the integrability of $udx+vdy+wdz$ indicates rectilinear motion, which I made at an early stage of my hydrodynamical researches, is adverted to by Professor Stokes in his Report on hydrodynamics contained in his recently published 'Mathematical and Physical Papers,' vol. i., and is with good reason objected to in page 161. I had not, at that time, arrived at the conception of

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the important distinction, indicated in the present communication, between spontaneous motion and motion produced under arbitrary conditions. The above assertion applies only to *spontaneous* motion. The recognition of this distinction suffices to meet completely Professor Stokes's objections.

At the end of a communication to the Philosophical Magazine for June 1880, I stated that I did not expect to have occasion to discuss any additional questions in Theoretical Physics. The importance and novelty of the present communication respecting spontaneous fluid-motion, the views respecting which have only very recently occurred to me, may, I think, be considered sufficient to justify my recurring once more to the subject of the Analytical Principles of Hydrodynamics.

Cambridge, February 21, 1881.

XXXV. *On the new Theory of Magnetic Attractions, and the Magnetic Rotation of Polarized Light.* By H. A. Rowland*.

IN a note published in the 'American Journal of Mathematics,' and also in the Philosophical Magazine for June 1880, I showed that the new action of magnetism on an electric current, recently discovered by Mr. Hall in my laboratory, was essentially of a rotational character, and I showed also that it was probably of the same nature as the rotation of the plane of polarization of light. I have since published a paper in the 'American Journal of Mathematics'—"On the General Equations of Electro-magnetic Action, with Applications to a new Theory of Magnetic Attractions, and to the Theory of the Magnetic Rotation of the Plane of Polarization of Light"—in which the subject is treated in full, and Maxwell's formula for the magnetic rotation of the plane of polarization deduced from the newly discovered action of magnetism.

Mr. Hopkinson has recently drawn attention† to the fact that Maxwell has inserted a certain "Rotatory Coefficient" in his equations of resistance ('Electricity,' art. 303). Maxwell further states that the coefficient should be found, if anywhere, in magnets, or, of course, in any magnetic field, as it has now been found. But I believe Maxwell nowhere connects this quantity with the rotation of the plane of polarization of light; and hence I think it may be well to give here an abstract of this portion of my paper.

In the 'Note' before referred to, I thought that it would be necessary to prove that the lines of electrostatic induction

* Communicated by the Author.

† Phil. Mag. Dec. 1880, p. 430.

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were rotated around the lines of force in the same way as the electric currents are rotated in metals; but I now find that this is not necessary, but that we only have to apply the rotation to the displacement currents of Maxwell, and we get a complete explanation of the magnetic rotation of the plane of polarization of light.

As Sir W. Thomson has shown that the latter effect must be due to some rotation in the magnetic field, so by connecting the new discovered action with the old, we prove that it also is due to rotation in the field. For Maxwell's equations merely express the facts of the case, and give no explanation.

Let a', b', c' be the components of the electric current in the direction of the axes, and a, b, c the components of the magnetic force. Let also $\mathfrak{c}$ be the coefficient of the new effect. Under these circumstances, electromotive forces will be set up in the medium whose components are

$$A'' = \mathfrak{c}(b_1c' - b'c_1),$$
$$B'' = \mathfrak{c}(c_1a' - c'a_1),$$
$$C'' = \mathfrak{c}(a_1b' - a'b_1).$$

To apply these results to Maxwell's theory of light, we must assume that the same action which takes place in conductors with reference to conducted currents, also takes place in dielectrics with reference to displacement currents. It is almost impossible to detect this action experimentally; but we shall here follow out the consequence of its existence. I shall follow the method of Art. 783 of Maxwell's 'Treatise,' with the addition of this new action*.

Assume at once $C=0$, $\psi=0$, and $J=0$, as they are afterwards taken or proved to be.

Let P, Q, and R be the components of the electromotive forces acting at any point. The electromotive force will be composed of two parts:—first, the rate of variation of the vector potential, as on the old theory; and, second, a term depending on the new action, and whose components we have designated by A'', B'', and C''. Adding these together, we have

$$P = -\frac{dF}{dt} + \mathfrak{c}(b_1c' - b'c_1).$$

$$Q = -\frac{dG}{dt} + \mathfrak{c}(c_1a' - c'a_1),$$

$$R = -\frac{dH}{dt} + \mathfrak{c}(a_1b' - a'b_1).$$

* I use the expression Δ^2 to signify the operation $\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}$, while Maxwell uses it (in his 'Theory of Light') with the opposite sign.

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The displacement currents a', b', and c' will be

$$a'=\frac{K}{4\pi}\frac{dP}{dt},$$

$$b'=\frac{K}{4\pi}\frac{dQ}{dt},$$

$$c'=\frac{K}{4\pi}\frac{dR}{dt};$$

and they are also expressed by the equations

$$4\pi\mu a'=-\Delta^2F,$$
$$4\pi\mu b'=-\Delta^2G,$$
$$4\pi\mu c'=-\Delta^2H.$$

Hence we have by elimination:—

$$K\mu\left\{\frac{d^2F}{dt^2}-\frac{d}{dt}c(b_1c'-b'c_1)\right\}-\Delta^2F=0;$$

$$K\mu\left\{\frac{d^2G}{dt^2}-\frac{d}{dt}c(c_1a'-c'a_1)\right\}-\Delta^2G=0;$$

$$K\mu\left\{\frac{d^2H}{dt^2}-\frac{d}{dt}c(a_1b'-a'b_1)\right\}-\Delta^2H=0.$$

Before the solution of these equations, of course the values of a_1, b_1, c_1, and a', b', c' must be substituted in terms of F, G, and H.

Let us now take the case of a plane-polarized ray passing in the direction of the axis of z, with a magnetic force c_1 along the same axis. The magnetic forces a, b, c, the variations of which constitute the waves of light, are very small; for Maxwell has calculated that in strong sunlight the maximum is about one tenth of the horizontal intensity of the earth's magnetism. Hence we can write

$$K\mu\left\{\frac{d^2F}{dt^2}+c\frac{d}{dt}(b'c_1)\right\}-\frac{d^2F}{dz^2}=0,$$

$$K\mu\left\{\frac{d^2G}{d^2t}-c\frac{d}{dt}(a'c_1)\right\}-\frac{d^2G}{dz^2}=0,$$

$$K\mu\frac{d^2H}{dt^2}=0,$$

and, replacing b' and a' by their values, we have

$$K\left\{\frac{d^2F}{dt^2}-\frac{cc_1}{4\pi\mu}\frac{d^3G}{dtdz^2}\right\}-\frac{d^2F}{dz^2}=0,$$

$$K\mu\left\{\frac{d^2G}{dt^2}+\frac{cc_1}{4\pi\mu}\frac{d^3F}{dtdz^2}\right\}-\frac{d^2G}{dz^2}=0.$$

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From the form of the equations we can well suppose that one solution is

$$F = r\cos(nt-qz)\cos mt,$$

$$G = r\cos(nt-qz)\sin mt;$$

and making the substitution we find

$$\left\{K\mu(n^2+m^2)-q^2\left(1+\mathfrak{c}\frac{mc_1K}{4\pi}\right)\right\}\cos(nt-qz)\cos mt$$

$$-Kn\left\{2m\mu-\mathfrak{c}\frac{c_1q^2}{4\pi}\right\}\sin(nt-qz)\sin mt=0,$$

$$\left\{K\mu(n^2+m^2)-q^2\left(1+\mathfrak{c}\frac{mc_1K}{4\pi}\right)\right\}\cos(nt-qz)\sin mt$$

$$+Kn\left\{2m\mu-\mathfrak{c}\frac{c_1q^2}{4\mu}\right\}\sin(nt-qz)\cos mt=0.$$

These are satisfied if we make the coefficients zero.

If V is the velocity in general of light in the medium, and V_0 the velocity *in vacuo* without magnetic action—if i is the index of refraction of the medium, and λ the complete wave-length in the medium, and λ_0 *in vacuo*, we thus find

$$m=\mathfrak{c}\frac{\pi c_1}{2\mu\lambda^2},$$

$$V=\frac{n}{q}=\sqrt{\frac{1+\mathfrak{c}\frac{Kmc_1}{8\pi}}{K\mu}},$$

$$V=\frac{1}{\sqrt{K\mu}}\left(1+\frac{K\mu m^2\lambda^2}{8\pi^2}\right).$$

These equations indicate that when a ray of plane-polarized light passes in the direction of the lines of magnetic force, the plane of polarization will be rotated in a direction depending on the sign of the quantity $\mathfrak{c}$, which is the well-known action of Faraday. But the second expression (which gives the velocity, and consequently the index of refraction) also depends on $\mathfrak{c}$, and thus indicates an acceleration of the velocity which is unknown. But this action is so very minute that it can probably never be measured.

If D is the length of the substance, the total angle of rotation of the beam will evidently be

$$\theta=m\frac{D}{V}=\mathfrak{c}\frac{\pi}{2\mu V_0}\frac{i^3}{\lambda_0^2}Dc_1.$$

This solution is rigorously exact for all cases where the index of refraction is not a function of the wave-length. To

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get the value where the index varies, we can use the principle of the superposition of small quantities. Every plane-polarized ray can be supposed to be made up of two circularly polarized rays; and to say that the plane of polarization is rotated simply means that one of the circularly polarized rays travels faster than the other; when one ray gains λ on the other, the plane of polarization is rotated through the angle 2π. Hence if V is the velocity of one and V′ of the other, we have

$$V = V'\left(1 + \frac{\lambda}{D'}\right),$$

where D′ is the distance in which the plane of polarization is rotated through the angle 2π.

But this effect will be augmented by the dispersion of the body, seeing that the velocity affects the wave-length, and hence the index of refraction will be different for the two components. This further action can be taken into account by multiplying $\frac{V}{V'}$ by $\frac{i''}{i}$; and we then have

$$\frac{V}{V'}\frac{i}{i''} = 1 + \frac{\lambda}{D''};$$

so that D′ has been changed to

$$D'' = \frac{\lambda}{\frac{i''}{i}\left(1 + \frac{\lambda}{D'}\right) - 1}.$$

This can be put into the form

$$\frac{D''}{D'} = \frac{1}{\frac{i''}{i}\left\{1 + \frac{D'}{\lambda}\frac{i'' - i}{i''}\right\}}.$$

But

$$\frac{\lambda}{\lambda'} = \frac{D' + \lambda}{D'},$$

whence

$$\lambda - \lambda' = \frac{\lambda^2}{D'};$$

and

$$\frac{i'' - i}{i} = \frac{\lambda' - \lambda}{i}\frac{di}{d\lambda} + \&c.$$

Hence, omitting all quantities of the second order of smallness, we can write

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$$\frac{D''}{D'} = \frac{1}{1-\frac{\lambda}{i}\frac{di}{d\lambda}};$$

and the angle of rotation θ will become

$$\theta = 2\pi\frac{D}{D''} = cDc_1\frac{\pi}{2\mu V_0}\frac{i^2}{\lambda_0^2}\left(i-\lambda\frac{di}{d\lambda}\right),$$

which is of the same form as Maxwell's expression. Now Maxwell's equation is obtained from considerations entirely different from any which I have used in this paper. In obtaining them, Maxwell made no assumption as to the kind of motion which constitutes light, but merely assumed that the magnetic lines of force were vortices, and that the motion of the vortices caused a rotation of the motion constituting light. In my theory I have used no hypothesis as to the nature of magnetic force; but have simply calculated, from the known laws of magnetism and electricity, the action in this case according to Maxwell's theory of light. And the conclusion which we draw is that *the effect discovered by* Mr. Hall *is the same, or due to the same cause, as the rotation of the plane of polarization of light.*

It is interesting to repeat here the comparison made by Verdet between the various formulæ and observation.

The formulæ of Maxwell and Rowland, of Airy, and of Neumann are

$$\theta = M\frac{i^2}{\lambda^2}\left(i-\lambda\frac{di}{d\lambda}\right)Dc_1, \quad \text{(I.)}$$

$$\theta = M\frac{1}{\lambda^2}\left(i-\lambda\frac{di}{d\lambda}\right)Dc_1, \quad \text{(II.)}$$

$$\theta = M\left(i-\lambda\frac{di}{d\lambda}\right)Dc_1. \quad \text{(III.)}$$

The comparison of these formulæ with the experiments of Verdet* are as follows:—

Bisulphide of Carbon.

	C.	D.	E.	F.	G.
Observed rotation .	0·592	0·768	1·000	1·234	1·704
Calculated, formula I.	0·589	0·760	1·000	1·234	1·713
„ „ II.	0·606	0·772	1·000	1·216	1·640
„ „ III.	0·943	0·967	1·000	1·034	1·091

* Verdet, *Œuvres*, vol. i. p. 262, or Maxwell's 'Electricity,' art. 830.

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Creosote.

	C.	D.	E.	F.	G.
Observed rotation .	0·573	0·758	1·000	1·241	1·723
Calculated, formula I.	0·617	0·780	1·000	1·210	1·603
„ „ II.	0·627	0·789	1·000	1·200	1·565
„ „ III.	0·976	0·993	1·000	1·017	1·041

To examine the direction of the action, we must see what the relative direction of the currents and magnetism are in the equations, as I have not taken the signs with respect to any system.

Let the positive direction of the current be the direction in which the positive electricity moves, and the positive direction of the magnetic lines of force be the direction in which the north pole tends to move; then we easily find that our equations are on the right-handed screw system, the right-handed screw being such that, if we turn it in the direction of the hands of a watch with its face towards us, it will move away from us. According to this system, Mr. Hall has found that the value of c is positive for gold and some other diamagnetic substances, and negative for iron. Hence a magnetic force in the positive direction will cause the ray to be rotated in the positive direction in diamagnetic substances, and in the negative direction in magnetic ones, which is exactly what has been observed.

To compare the numerical amount of the revolution with observation, we can take the constants as observed by Mr. Hall for gold, and thus find at least whether it is of the proper order of magnitude.

From more recent observations than those published, Mr. Hall finds that in the field of his magnet he can cause the lateral electromotive force to be at least as great as $\frac{1}{2000}$ of the force along the strip. According to the system of units used in this paper, the new electromotive force will be (in the case of conduction, the current passing along Y and the magnetism being in the direction of z)

$$A'' = -cc_1b' = -4\pi\mu' cc_1 B';$$

but Mr. Hall finds

$$\frac{B'}{A''} = -2000, \text{ nearly.}$$

Hence, using the C.G.S. system, in which $\mu' = 2000$ nearly, we shall have

$$cc_1 = \frac{1}{4\pi}, \text{ nearly, for gold.}$$

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The length of the substance in which the ray is rotated a complete revolution, or 360°, will then be

$$D=\frac{2\pi V_0}{mi}=\frac{4\mu\lambda_0{}^2V_0}{cc_1i^2\left(i-\lambda\frac{di}{d\lambda}\right)},$$

where it is to be noted that λ_0 is the length of a *complete* wave. Taking the wave of $\frac{1}{10000}$ centim. length and the index of refraction 4, we find, supposing $\frac{di}{d\lambda}=0$,

$$D=240 \text{ centims., nearly.}$$

We do not know the magnetic force used by Verdet; but it was evidently of the same order of magnitude. He found D to be about as follows—300 for heavy glass, 700 for flint glass. Hence the rotation calculated for gold is of the same order of magnitude as the rotation observed in some common substances.

Thus the new electromagnetic phenomenon explains in the most perfect manner the magnetic rotation of the plane of polarization of light; and we are almost in the position to pronounce positively that the two phenomena are the same. Should this preliminary theory of the subject stand the test of time, it hardly seems to me that we can regard it in any other light than a demonstration of the truth of Maxwell's theory of light; for the rotation of the plane of polarization is thus a necessary consequence of the laws of electromagnetism, and this, added to the other facts of the case, raises Maxwell's theory almost to the realm of fact.

XXXVI. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part IV. *By* C. R. Alder Wright, *D.Sc. (Lond.), Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School.*

[Plate III.]

On the Validity of Faraday's Law as to the Proportionality between the Mass Electrolysed and the Quantity of Electricity passing; on the Numerical Relationships between the Counter Electromotive Force set up by weak Currents and the Rate of Flow of these Currents; and on the Conditions influencing these Relationships and the consequent Dependence of the E.M.F. of certain forms of Electromotors on the Currents generated by them.

77. THE original experiments of Faraday and of many subsequent observers have amply established, for moderately strong currents, the truth of the proposition

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known as "Faraday's law," or as "Faraday's first law of electrolysis" (Part I. § 7), viz. that when a compound is electrolysed the weight of substance decomposed is sensibly proportionate to the quantity of electricity passing. Still, not only Faraday himself, but also subsequent experimenters have been led to doubt whether this law is rigorously true in all cases, *i. e.* whether it is not possible for very weak currents to pass through certain electrolytes without actually causing electrolysis, or at least without causing an amount of electrolysis precisely proportionate to the quantity of electricity passing. In other words, it has been held that "conduction without electrolysis" may occur in certain cases. On examining the experimental evidence in support of the doubt, however, it does not seem to be at all proved in the cases in which no electrolysis was apparent, even though a weak current passed, that electrolysis did not actually take place, the products being prevented from becoming visible owing either to secondary chemical changes, or other circumstances. More especially in the case of electrolysis of water is this deficiency of proof noticeable; for if the liquid electrolysed were in contact with air before ordinary electrolysis, the hydrogen evolved would necessarily be more or less completely suppressed by the oxidizing action of dissolved oxygen; whilst even in the case of experiments with boiled fluids, or fluids *in vacuo*, the impossibility of getting rid of the last traces of oxygen dissolved in the fluid or occluded in the electrodes &c. would still affect the quantity of free hydrogen collected, as would also the power of the electrodes to occlude within their material, or to condense upon their surface, the hydrogen that would have appeared in the free state could these actions of the electrodes have been completely eliminated. Moreover it was shown long ago by Buff (*Ann. der Chem. und Pharm.* lxxxv. p. 1, and xciv. p. 15, 1853 and 1855) that currents in the ratio of $\frac{1}{R}, \frac{1}{R+r}, \frac{1}{R+2r}$, and $\frac{1}{R+4r}$, when passed through silver solutions deposited quantities of silver sensibly in the same ratio, even when the currents were so minute that the smallest of them only deposited 0·129 gramme of silver in 100 hours, and hence deposited $\frac{0{\cdot}129}{100\times 60\times 60}$ grammes per second, corresponding to a C.G.S. current of $\frac{0{\cdot}129}{360{,}000\times 0{\cdot}01163}=0{\cdot}0000308$, or to 0·000308 weber (1 weber = 0·1 C.G.S. current-unit), since 1 C.G.S. current-unit deposits 0·011363 gramme of silver (Part I. § 9). Similarly Buff found that currents capable of depositing

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from ·009 to ·113 gramme of silver per 100 hours (·000022 to ·00027 weber) evolved quantities of hydrogen usually only differing by less than ·05 cubic centim. from the quantities calculated from the current passing when air-free solutions of Glauber's salt, dilute sulphuric acid, or distilled water were electrolysed for from 4½ to 26 hours, so that from 0·11 to 1·09 cubic centim. were collected, the hydrogen pole being a Wollaston point; whilst a just visible evolution of hydrogen was noticeable from a Wollaston point with a yet smaller current, capable of depositing ·001 gramme of silver per 100 hours, and hence equal to ·0000024 weber. Whilst these experiments indicate that conduction without electrolysis did not take place to any appreciable extent during the electrolysis of the silver salt used, they are yet scarcely precise enough to establish with certainty the same point in the case of water, although they clearly point in that direction. Accordingly the following experiments were made with a view to supplementing Buff's work in this respect.

78. Voltameters were constructed like the one represented in Pl. III. fig. 1. A U-tube, AB, was drawn out and cut off in the middle of one of the limbs, and the drawn-out part passed through an indiarubber cork, C, smeared internally with melted rubber to make an air-tight joint. Over this cork (similarly smeared outside) was fixed a calibrated capillary tube widened into a sort of thistle-funnel or bell-mouth D, at the part into which the cork fitted; this tube was bent to a double right angle, so that the part used for measurement, EF, pointed downwards, the lower end passing through an indiarubber cork, H, into a second U-tube, G, the other end of which was closed by another rubber cork through which a stout glass rod, I, passed. The upper U-tube was provided with electrodes, K and L, of platinum-foil of measured size fixed to platinum wires fused through the glass (or sometimes, in the case of the longer limb, supported, as represented in the figure, by a loosely fitting cork in the open mouth of the tube), by means of a platinum wire sealed through a drawn-out glass tube, M, which, being filled with mercury, served as a mercury-cup. In order to fill the instrument, the corks supporting I, and also H, were loosened, and recently boiled, still hot, dilute sulphuric acid poured into the longer limb of the U-tube, AB. This acid rapidly filled this U-tube, and then passed over through the capillary tube, DEF, into the lower U-tube, G, also filling it. A certain amount of acid was allowed to run through the whole instrument, so as to rinse out the first portion poured in, which had probably absorbed air to some extent, after which the cork

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supporting I was firmly fixed, and then the cork H. If now the two electrodes were connected with a battery, evidently the gases evolved would arise, the one into the bell-mouth of this capillary tube, the other to the surface of the fluid in the longer limb, B, of the upper U-tube. By making K the — electrode, and by working the glass rod I up or down through the cork supporting it, the hydrogen evolved could either be drawn over into the measuring part EF, or forced back again into D; and in this way measurements of the gas contained in D could be readily made. When the quantity of gas collected became too large for measurement, it could be drawn completely over into the lower U-tube, G, so as to empty the bell and capillary tube of gas, by simply drawing out the rod I.

79. In order to saturate the acid in the two limbs A and B with hydrogen and oxygen respectively, as well as to do away with the sources of loss of hydrogen due to traces of residual air and occlusion by the electrode K, a weak current (about ·0000025 to ·00005 C.G.S. current-unit*, capable of evolving from about 0·2 to 0·5 cubic centim. of hydrogen per day) was passed through the apparatus for several days or even two or three weeks, the evolved hydrogen being from time to time drawn over into the lower U-tube G, and expelled by cautiously loosening the cork H for an instant or two. If during this time measurements were made of the quantity of hydrogen actually collected as compared with that calculated from the current passing (kept as nearly as possible uniform by suitably altering the resistance in circuit, and measured by determining the potential difference set up between the ends of that resistance, as described in Part III. § 69), it was found that a deficiency in the amount collected was always observed, but that that deficiency gradually decreased to a constant minimum. Moreover the period when this constant minimum was first reached was found to be precisely the period at which the rate of loss of polarization observed on breaking circuit reached its constant minimum (Part III. § 70); and it was also precisely the period when the counter E.M.F. set up by a given current with a given apparatus ceased to increase (§ 85). These coincidences, as well as *à priori* reasoning, leave no room for doubt that the cause of the residual constant discrepancy between the observed and calculated amounts

* The currents used in the majority of the experiments described below being only small fractions of a weber, it is more convenient to measure them in millionths of a weber, or *microwebers*. One microweber = 0·000001 weber = ·0000001 C.G.S. current-unit; so that the above currents are respectively 25 and 50 microwebers.

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of hydrogen is due to that cause of loss of hydrogen which cannot possibly be entirely removed, viz. the diffusion to the hydrogen plate of oxygen-containing liquid, and *vice versâ*; whilst the larger deficiencies at first noticed are due to this cause increased by the solution of hydrogen in the fluid, its occlusion by the electrode, and its oxidation by the last traces of unremoved air. That this is the true cause of the residual constant deficiency is moreover further indicated by the fact that the nearer together the electrodes and the wider the bore of the U-tube, AB (that is, the more rapid the rate of diffusion), the greater, *cæteris paribus*, is the residual deficiency; whilst fluctuations of temperature, which would necessarily aid the diffusion of the gaseous solutions by setting up convection currents, considerably increase the deficiency. It might be supposed that perceptible errors in measurement of the hydrogen would be occasioned by the adherence to the electrode K of visible minute bubbles of gas; in practice, however, it was always found that gently tapping the U-tube AB caused such bubbles to rise so completely into the bell D, that no visible bubbles still adherent to K could be distinguished.

80. In order to determine quantitatively the amount of loss of hydrogen due to this unavoidable "diffusion discharge" (as for want of a better name it may be termed), a large number of observations were made with the different voltameters employed in the following way. Firstly, a current of 25 to 50 microwebers was passed until the residual deficiency became constant (for a given current); and then the current was gradually diminished at stages of two or three days apart, determinations of the hydrogen actually evolved per day, or per two or three days, being made. By-and-by a particular limiting current-value was arrived at, such that with the particular voltameter used no gas whatever was collected, no matter how long the current was allowed to pass, if the current-strength were below that limiting value; whilst if the current were above that amount, and provided the temperature was as nearly uniform as circumstances permitted, so that this effect of heat-convection currents was reduced to a minimum, a quantity of hydrogen was collected after a given time, the greater the larger the current-value. In this way the following limiting values were obtained with four voltameters, some of the particulars of the construction of which are annexed:—

Voltameter No. 1. Electrodes of platinum-foil bent into cylinders so as to be about concentric with the walls of the U-tube. Total surface of each electrode (reckoning both sides

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of the cylinder and including that of the piece of platinum wire connected with it to make contact) 27·5 square centims. Average bore of U-tube between the electrodes round the bend = 1·50 square centim. in section. Distance apart of centres of cylindrical electrodes = 13·0 centims.

Limiting value of current, expressing the rate of diffusion discharge not seriously increased by effects of variation of temperature, = 4·0 microwebers, capable of evolving about 0·04 cubic centim. of hydrogen at 0° and 760 millims. in 24 hours.

Voltameter No. 2. Similar electrodes, each exposing 8·0 square centims. of total surface. Centres of electrodes 8·0 centims apart. Average bore of tube 1·0 square centim. in section.

Limiting value of current 8·0 microwebers.

Voltameter No. 3. Electrodes of foil, each exposing 3·2 square centims. total surface. Centres of electrodes 6 centims. apart. Average bore 1·1 square centim.

Limiting current-value 5·0 microwebers.

Voltameter No. 4. Electrodes of platinum wire, each exposing 0·19 square centim. of total surface. Distance apart 17 centims. Average bore of tube 0·08 square centim.

Limiting current-value 1·5 microweber.

Voltameters Nos. 1, 3, and 4 contained acid of which 100 cubic centims. contained 27·0 grammes of H_2SO_4. In these three cases the values of the limiting currents are evidently in the order of the facility with which diffusion would go on—the column of fluid between the electrodes being respectively 13, 6, and 17 centims. long, the first being widest and the last narrowest, whilst the current values are respectively 4·0, 5·0, and 1·5 microweber. The relatively larger value for the limiting current of voltameter No. 2 is explained by the fact that it contained a more dilute and consequently less viscid acid, 100 cubic centims. containing only 1·0 gramme of H_2SO_4.

The amount of current due to imperfect insulation of the voltameter &c. in these experiments was wholly inappreciable. On connecting the voltameter emptied of acid with a battery of two or three Minotti cells and a resistance of 100,000 ohms, no leakage-current at all could be detected; that is, the difference of potential between the ends of the resistance of 100,000 ohms was less than ·005 volt (half a scale-division of the quadrant-electrometer); so that the leakage-current was less than $\frac{\cdot 005 \times 10^8}{100{,}000 \times 10^8}$, *i. e.* less than 0·05 microweber, a current which would require upwards of five years to evolve 1 cubic centim. of hydrogen from water.

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81. The average current passing during any given experiment was determined as follows:—The voltameter having been arranged and the current turned on at a noted time, the difference of potential set up by the current passing between the ends of a known resistance (corrected when necessary for temperature) was determined by means of the quadrant-electrometer; and hence the current passing was known. During the first two or three hours the current usually diminished somewhat, owing to the increasing counter E.M.F. set up in the voltameter; but subsequently it remained sensibly steady, under suitable conditions as to battery-power, temperature, &c. By means of numerous observations the average currents passing in each of various successive periods of time were known; and hence the average current for the whole time was known. Thus, for example, the average currents in one experiment were as follows:—

Period.	Average current during period.
Commencement to 6 hours.	22·4 microwebers.
6 hours to 21 hours	22·1 „
21 „ 29 „	21·5 „

Hence the average current for the whole period is

$$\frac{6\times 22{\cdot}4+15\times 22{\cdot}1+8\times 21{\cdot}5}{29}=22{\cdot}03 \text{ microwebers.}$$

In standardizing the quadrant-electrometer, use was made either of a moderately newly constructed Clark's cell, or of one which had "run down" to a known extent as compared with a newly prepared cell*, the E.M.F. of a moderately new cell being taken from Clark's experiments to be

$$1{\cdot}457\{1-(t-15)\times 0{\cdot}0006\} \text{ volt}$$

at $t°$ (*i. e.* the difference of potential set up by the cell between two poles of the same material being expressed by this amount). It hence results that if the value of the B.A. unit of resistance (on which the valuation of the E.M.F. of Clark's cell depends,

* A large number of experiments made with Clark's cells showed that whilst the E.M.F. of comparatively newly prepared cells (*i. e.* a few days to some three months or so old) is sensibly constant *when suitable precautions are taken in the construction*, yet a gradual decrease in the E.M.F. begins to take place after a longer or shorter time and then goes on continuously. Experiments on the construction and the circumstances influencing the permanence of these cells and others of different kinds, are in progress, which will be described in a future paper; it may be noticed, however, that cells prepared with mercurous-sulphate paste, boiled in a Sprengel vacuum and enclosed in hermetically-sealed glass tubes free from air, appear to be preferable to cells prepared in accordance with Clark's directions with a sealing of melted paraffin-wax (Proc. Roy. Soc. xx. p. 444, 1872).

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Part II. § 54) be affected by any error, the values assigned to the current passing in the present experiments are also erroneous in the same ratio. In the majority of the experiments described below the difference in the calculated amount of hydrogen, according as the B.A. unit is assumed correct, or as being $\pm 0{\cdot}5$ or even ± 1 per cent. in error, is less than the error of measurement of the gas collected, together with the error due to the rate of "diffusion discharge" not being absolutely constantly equal to the values given above, owing to unavoidable fluctuations of temperature.

82. By comparing the quantity of gas collected after the passage of a known average current for a known time with the amount calculated from the current and time by the formula

$$w=(C-C')t\chi$$

(where w is the weight of the hydrogen, C the average current passing, C′ the current equivalent to the "diffusion discharge," and t the time in seconds), numbers were obtained according closely together: thus, for example, the observed quantities of hydrogen collected in the experiment cited in § 81 were as follows (voltameter No. 1 used):—

Period.	Milligramme of hydrogen.
Commencement to 6 hours.	0·004
6 hours to 21 hours.	0·0105
21 „ 29 „	0·0055
Total . .	0·0200

whilst the amount calculated from the formula is

$$(0{\cdot}000002203-0{\cdot}0000004)\times 29\times 60\times 60\times 0{\cdot}000105 \text{ gramme}=0{\cdot}0198 \text{ milligramme};$$

or sensibly the calculated amount was actually obtained, the difference between the two quantities being only 0·0002 milligramme of hydrogen, representing about 0·002 cubic centim. at 0° and 760 millims., an amount well within the experimental errors.

In precisely the same way the following numbers were obtained in a series of experiments with the same voltameter (No. 1). The currents are given in microwebers (1 microweber $=0{\cdot}0000001$, or 1×10^{-7} C.G.S. current-unit) in the first column, and the time in hours in the second; the third column gives the value of $Ct\chi$, and the fourth of $(C-C')t\chi$, the two sets of values being given in order that the amount of suppression of hydrogen due to "diffusion discharge" may be rendered manifest.

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Average current, in microwebers.	Time, in hours.	Weight of hydrogen, in milligrammes.		
		Total calculated from quantity of electricity passing.	Previous amount, less that due to "diffusion discharge."	Observed amount.
380	30	·432	·428	·43
285	20	·216	·213	·21
200	23	·174	·170	·17
90	24	·082	·078	·076
80	42	·127	·121	·120
48	17	·030	·028	·027
47	42	·075	·069	·068
24	22	·020	·017	·017
22·0	29	·024	·020	·020
9·7	43	·016	·009	·009
4·7	48	·009	·001	·001
Less than 4·0	Various periods up to 192 hours.	——	nil.	nil.

Similarly, the following numbers were obtained with voltameter No. 2:—

750	1·25	·050	·049	·049
285	6·25	·067	·065	·063
191	5·25	·038	·036	·035
122	4	·018	·017	·016
113	21·25	·090	·084	·081
59	22·5	·050	·043	·041
34	23·5	·029	·022	·022
21·0	23	·018	·010	·010
17·0	55	·036	·018	·019
16·3	46	·030	·016	·016
Less than 8·0	Various periods up to 100 hours.	——	nil.	nil.

With voltameter No. 3 the following numbers were obtained:—

220	16	·133	·130	·130
65	24	·052	·048	·045
50	47	·089	·081	·080
6·3	41	·010	·003	·004
Less than 5·0	Various periods up to 240 hours.	——	nil.	nil.

And with voltameter No. 4 the following numbers:—

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23·0	21	·018	·017	·017
20·0	24	·018	·017	·017
7·0	46	·012	·009	·009
Less than 1·5	Various periods up to 100 hours.	——	nil.	nil.

In nearly every instance the observed quantities of hydrogen and those calculated from the quantity of electricity passing, after subtracting the amounts due to "diffusion discharge," either coincide or do not differ by more than ·001 milligramme of hydrogen, or about 0·01 cubic centim., an amount of variation not outside the probable errors of measurement. In the few cases where the difference reaches ·002 or ·003 milligramme, the observed amount is always less than the calculated quantity, the cause being that the temperature could not be kept absolutely uniform, and consequently the loss of hydrogen by "diffusion discharge" is a little underestimated, the normal value of the rate of diffusion discharge being a little increased through the effects of heat-convection (§ 79). Some half dozen other experiments were made in which slightly greater differences were observed, always in the same direction, viz. deficiency in the amount of hydrogen collected; in these experiments the temperature fluctuated more than in those above cited, and hence the results are not quoted, being vitiated by the increased effect of heat-convection.

From these numbers the final conclusion may be fairly drawn, that *when water is electrolysed the mass decomposed is always proportionate to the quantity of electricity passing*, no matter how great may be the time it takes to pass, *i. e.* no matter how small is the actual current employed, thus comfirming and amplifying the results obtained by Buff above quoted; so that the universal validity of Faraday's law may fairly be taken as being established. On the other hand, the recent experiments of Guthrie and Boys (Phil. Mag. [5] x. p. 328, Nov. 1880) show that a glass vessel filled with dilute sulphuric acid or other electrolyte, and suspended by a wire in a rotating magnetic field, is urged to a move by a force producing torsion in the wire varying as the product of the conductivity of the electrolyte into the relative speed of the magnets compared with that of the fluid set rotating whirlpool-fashion inside the glass vessel by induction; and it is supposed by the authors that, in this case, no electrolysis occurs, although currents circulate in the fluid. The absence of electrolysis, however, in these experiments, is not only unproved but is highly improbable. At any given instant any two opposite points on the inner surface of the glass con-

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taining-vessel in contact with the liquid will exhibit a greater or less difference of potential, and a current will flow between them through the liquid, the points on the glass surface acting as electrodes ; after a half revolution the potentials of the two points are reversed, the current flowing in the opposite direction relatively to the points (but in the same direction relatively to the magnets) ; so that so far as these two points are concerned a series of alternating currents (or rather of currents going through a regular cycle increasing from 0 to a + maximum, decreasing to 0 and then to a − maximum, and then increasing again to 0, and so on) are set up, the effect of which is to eliminate the effects of "polarization" as completely as in Kohlrausch's method of determining conductivity by means of alternating currents, the products of electrolysis being reunited, so to speak, by the action of the reverse current before they have time to escape. Since the nett chemical action is nil, the work done by the currents appears solely as heat ; but it does not therefore follow that electricity passes through the liquid without causing electrolysis, any more than in Kohlrausch's experiments.

Relationships between the Counter Electromotive Force set up during Electrolysis and the Rate of Flow of the Current passing.

83. It has often been noticed by previous observers that the so-called "polarization," or, more strictly, counter E.M.F. (§ 57) set up during electrolysis, especially of acidulated water, by a given current, gradually rises until it becomes approximately constant. The cause of this is doubtless that so long as the fluid surrounding the hydrogen-electrode contains dissolved air, and so long as the electrodes and the fluids surrounding them are capable of further attracting, occluding, or dissolving the gases evolved in their proximity, the aura surrounding each electrode does not possess its normal average density ; that is, the values of $\Sigma(nh)$ and $\Sigma(\mathfrak{H})$, in the expression for the counter E.M.F. set up (Part III. § 61),

$$e=\mathrm{E}_1+[\Sigma\{(1-n)\mathrm{H}-\Sigma(nh)-\Sigma(\mathfrak{H})]\chi\mathrm{J},$$

are greater at first than the values to which they finally subside.

For the same reason, if an electromotor of E.M.F not greater than 1 to 1·5 volt or so be connected with a voltameter containing diluted sulphuric acid (unboiled, or even if boiled), and provided with electrodes of non-oxidizable material, as is well known, a current flows at first, the strength of which gradually diminishes to a minute amount, whilst the so-called

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"polarization" of the voltameter increases until it differs from the E.M.F. of the electromotor by only a very minute quantity. With boiled acid, and especially in voltameters so constructed that neither the oxygen evolved at the + electrode nor the external air can reach the − electrode (saving by unavoidable diffusion), the rate of the diminution of the current is much greater than is the case when these conditions are not fulfilled. On trying experiments of this class, however, it was found that even with the most carefully constructed voltameters the current never became too small to measure, even after many weeks; it gradually subsided to a limiting value, below which it never sank (*i. e.* as long as the E.M.F. of the electromotor and the resistance of the circuit remained the same). No trace of gas, however, was evolved at either electrode; which was evidently due to the fact that, owing to "diffusion discharge," the hydrogen and oxygen evolved by the current became reconverted into water just as rapidly as they were generated by the current; so that the particular final steady current attained with any given apparatus measured the rate of diffusion discharge with that apparatus under the particular conditions of the experiment.

84. On comparing such results with different electromotors, it was at once perceived that when the resistance in circuit remained the same, the greater the E.M.F. of the electromotor the greater was the value of the final steady current that passed, although as long as the E.M.F. of the electromotor did not exceed a certain limiting value (different for each form of apparatus) no visible evolution of gas took place in any case; *pari passu* with the increase in the strength of the current, the counter E.M.F. set up was found to increase; that is, the value of $e = E - CR$, where E is the difference of potential between the electrodes, C the current, and R the resistance of the column of fluid between the electrodes (Part III. § 57). Thus the following values were obtained with voltameter No. 1 above described (§ 80), and precisely analogous ones with the others.

Final steady current.	Value of e.
0·1 microweber.	0·140 volt.
0·13 „	0·297 „
0·24 „	0·468 „
0·5 „	0·615 „
0·8 „	0·929 „
1·2 „	1·395 „
2·3 microwebers	1·523 „
3·1 „	1·564 „
4·0 „	1·663 „

With this particular voltameter currents of greater magnitude

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than four microwebers caused an evolution of hydrogen (§ 80); with currents less than this amount none was obtained. Since the values of the counter electromotive forces set up by these currents were considerable, especially when the current was near the limiting value of four microwebers, it results that the hydrogen which combined with the oxygen diffusing to the — electrode, and the oxygen combining with the hydrogen that diffused to the + electrode, was the gas occluded within or condensed upon each electrode respectively, and not that produced by the electrolysis reoxidized *pari passu* with its formation; for in the latter case the nett work done as electrolysis during the passage of this current would be *nil*, and hence e would $=0$. The action of the "diffusion discharge" is therefore precisely like that of dissolved air present in the fluid electrolysed in the first instance (§ 76), comparable with the "local action" of an ordinary cell. In any given experiment the value of e will rise (the current remaining constant) until the rate of diminution of aura at each electrode due to this cause just balances the rate of increase through electrolysis. As this latter rises when the current is increased, so will the mean aura-density rise, and the amount of aura dissolved away from the electrode by the surrounding fluid in a given time, and consequently the rate of diffusion discharge, until the aura is at its maximum mean density, *i. e.* saturated, after which a further increase of the current simply causes free gases to escape. In this case the value of e is still found to increase with the current, obviously indicating that the values of the n's (*i. e.* of the fractions of hydrogen and oxygen not evolved in the nascent state *ab initio*) decrease as the current increases. Similarly, when the temperature falls the value of e rises (*cæteris paribus*) and *vice versâ*, indicating that a fall in temperature increases, and a rise decreases, the fractions of gases evolved "nascent" in the first instance.

The following experiments were then made with the view of investigating the effects produced in the value of e (that is, in the values of the n's) by altering certain of the conditions of electrolysis.

Effect of Alteration of the Size of the Electrodes.

85. It would seem *à priori* probable that whilst e increases with the current when the electrodes are the same, and decreases when the size of the electrodes is increased, the current remaining the same, it will remain constant if the current and size of electrode increase in the same ratio, so that the rate of flow of electricity per unit of electrode-surface (sometimes

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spoken of as the "density of the current") is constant. The following experiments show that this is so.

The voltameters used were mostly those described above (§ 78), or others of similar construction, only differing in that the measuring-tubes were replaced by ordinary fine tubes bent over and dipping under mercury or water, so as to allow of the evolved gas escaping without introduction of air—a similar arrangement being applied at the limb containing the + electrode (as described in Part III. § 71). They were filled with boiled dilute sulphuric acid of such strength that 100 cubic centims. contained 27·0 grammes of H_2SO_4 (in other experiments different strengths of acid were employed), and then submitted to the action of a current of 25 to 50 microwebers for several days, or even weeks, until the rate of fall of the polarization noticed after breaking battery-contact became sensibly constant at its minimum value for the particular current passing (§ 70), at which time, as already stated (§ 79), the difference between the hydrogen actually collected and that calculated from the quantity of electricity passing also became reduced to the constant minimum. During the time that elapsed since first starting the experiment, observations of E, the difference of potential between the electrodes, were frequently made: the current C passing being also determined by observing the difference of potential set up between the ends of a known resistance also in circuit (Part III. § 69), and the resistance R of the voltameter being known from the dimensions of the column of liquid between the electrodes and its specific resistance (as determined by Kohlrausch and others), the value of $e = E - CR$, the counter E.M.F. set up, was known. It was invariably found that the value of e continually rose after first starting the experiment until it finally reached a constant maximum, the time when this maximum was reached being precisely coincident with the epoch when the minimum rate of fall of polarization and the minimum deficiency in hydrogen collected were attained.

Thus, for example, the following numbers were obtained with voltameter No. 1, when filled with acid containing 40·0 grammes of H_2SO_4 per 100 cubic centims., the values being calculated by interpolation from experiments in which the current was uniformly near to the value 1000 microwebers = 0·0001 C.G.S. current-unit.

Counter E.M.F. set up for a current of 1000 microwebers.

Freshly boiled acid just cool; values obtained during first few minutes of passage of current.	Values gradually increasing up to 1·965 volt.

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After passage of a current of about 300 microwebers for two days, so as to evolve some 5 cubic centims. of gaseous hydrogen, and nearly saturate the voltameter-liquid with oxygen and hydrogen in the two limbs respectively.	Values gradually rising from 1·965 to 2·150 volts.
After another week's passage of a current of about 50 microwebers.	Values rising from 2·150 to 2·250 volts.
Constant maximum attained after a fortnight's passage of the current.	2·267 volts.

86. In precisely the same way analogous numbers were obtained with the same voltameter and different currents, or different strengths of acid, and similarly with the other voltameters. It was found that when the sources of loss of hydrogen other than diffusion discharge were wholly eliminated, the value of the counter E.M.F. set up with a given current not only remained constant as long as the current did not vary, but also was the same if the current was varied through a considerable length of time (whilst making other observations), and then brought back to the original amount. With feeble currents of strength not exceeding a small number of microwebers per square centimetre of electrode-surface, however, the value of e was found to be very sensitive to causes which would act on the equilibrium of the auræ surrounding the electrodes, especially to changes of temperature which would increase the normal effect of diffusion discharge by setting up heat-convection currents; so that the constant E.M.F. due to the original current was often not absolutely (but only approximately) arrived at until after the lapse of some hours. For this reason too, the counter E.M.F. observed was often less by 1 or even 2 or more per cent. in such cases when determined in the afternoon (in the warmest part of the day) than when determined the first thing in the morning—partly because the actual rise of temperature diminished its value, but more particularly because of the increased effect of heat-convection in affecting the auræ. In making the following determinations, therefore, the mode of procedure adopted was as follows:—The voltameter having been saturated with hydrogen and oxygen in the two limbs respectively by a fortnight's passage of a current of some 50 or 100 microwebers, a particular current was adjusted to pass by applying a given battery (of Minotti, De la Rue and Müller, Leclanché, or other cells, as most convenient, but chiefly the three first), and introducing a given resistance into the circuit to measure the current with. The whole was then left to itself till the next

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day, when a number of careful observations of the current passing and the value of E were made at as early an hour as possible (usually about 9 o'clock), so that the effect of the heat-convection currents due to alteration of laboratory temperature should be a minimum. The particular temperature of the measuring resistance-coil (platinum-silver or German-silver wire) being known, its resistance in ohms R_1 was known; and the potential-difference in volts E′ being known by standardizing the scale of the electrometer by a Clark's cell, the current was known in C.G.S. units, being given by the formula $C=\frac{E' \times 10^8}{R_1 \times 10^9}$. The resistance of the voltameter, R, being known, the value of $e=E-CR$ was thus known (usually the term CR was negligible, or at least small compared with E). The current was then allowed to pass for 24 hours, and the observation repeated as early as possible in the day, and so on. Usually the results of consecutive days' measurements agreed extremely closely. When a sufficient number of daily determinations had been made, the resistance in circuit was altered so that a different current passed, and another series of determinations lasting over some days made, and so on. Sometimes after making various series of determinations of the relative values of C and e in this way, the value of C was brought back to nearly the same value as that possessed in observations of several days or weeks before. In such cases the values deduced in the second set of observations were sensibly the same as those formerly obtained; or at least they uniformly corresponded to points lying sensibly on the same mean curve, obtained by plotting the currents as abscissæ and the values of e as ordinates. Thus, for instance, the average of three closely accordant sets of consecutive morning readings with voltameter no. 1 (containing acid of which 100 cub. centims. contained 40·0 grammes H_2SO_4), made near the beginning of a long series of determinations lasting over many weeks, and that of three other sets of accordant observations made near the end of the period, gave the following numbers—

Earlier series.		Later series.	
Current in micro-webers.	e in volts.	Current in micro-webers.	e in volts.
23·5	1·906	22·3	1·901
21·0	1·892	20·1	1·895
20·6	1·880	19·8	1·870
Average 21·7	1·893	20·7	1·889

thus giving the final average—Current = 21·2, e = 1·891.

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Similarly with voltameter no. 4, containing acid of which 100 cubic centims. contained 27·0 grammes H_2SO_4, the averages of two similar series of daily observations, near the beginning and end respectively of a lengthened period of time during which the curve-values were being determined, were as follows :—

	Current in microwebers.	*e* in volts.
General average from early series.....	4·70	2·334
„ „ „ later „	4·85	2·337
Mean.....	4·77	2·3355

and so on in dozens of other analogous cases.

87. The following tables give the final averages deduced from four lengthened series of daily morning observations made with voltameters nos. 1, 2, 3, and 4 above mentioned (§ 80), containing acid of such strength that 100 cub. centims. contained 27·0 grammes of H_2SO_4. The average temperature of the different series of experiments was in each case not far from 18°, near to which temperature the specific resistance of dilute sulphuric acid was found to be as follows by Kohlrausch and Nippoldt (Pogg. *Ann.* cxxxviii. p. 379, 1869):—

Sp. gr. of acid.	Percentage of H_2SO_4.	H_2SO_4 per 100 cubic centimetres.	Specific resistance at 22°.	
			Mercury = 1.	Absolute (C.G.S. system).
		grammes.		
1·1431	20·2	23·1	14990	$1·424 \times 10^9$
1·2045	28·0	33·7	13133	$1·247 \times 10^9$

the numbers in the last column being deduced from those in the preceding one by taking the specific resistance of mercury on the C.G.S. system as being $0·000095 \times 10^9$, *i. e.* 0·000095 ohm per column of 1 square centim. in section and 1 centim. long; whence, by interpolation, the specific resistance of acid containing 27·0 grammes H_2SO_4 per 100 cub. centims. is, at 22°, $1·31 \times 10^9$. The numbers obtained by Kohlrausch (Pogg. *Ann.* Ergbd. viii. p. 1, and clix. p. 233, 1876) yield a slightly different value, about 12 per cent. higher.

Sp. gr. of acid.	Percentage of H_2SO_4.	H_2SO_4 per 100 cubic centimetres.	Conductivity at 18°. Mercury = 1.	Specific resistance at 18° (C.G.S. system).
		grammes.		
1·1414	20	22·8	·00006108	$1·556 \times 10^9$
1·1807	25	29·5	·00006700	$1·417 \times 10^9$

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whence acid of 27·0 grammes per 100 cub. centims. would have a specific resistance of $1{\cdot}47 \times 10^9$ at 18°.

The value obtained by Paalzow for acid represented by H_2SO_4, 14 aq. (Pogg. *Ann.* cxxxvi. p. 489, 1869), was 14157 times the resistance of mercury at 19°. This acid contains 28·0 per cent. of H_2SO_4, representing 33·7 grammes per 100 cub. centims., possessing the specific resistance $1{\cdot}34 \times 10^9$, or about 8 per cent. higher than that found by Kohlrausch and Nippoldt.

On the whole the specific resistance of acid of the strength used, viz. 27 grammes per 100 cub. centims., may be taken as 1·4 ohm at 18°, whence the values of R for the four voltameters, calculated from the dimensions of the columns of fluid between the electrodes (§ 80), are :—

Voltameter No. 1. $\frac{13 \times 1{\cdot}4}{1{\cdot}5}$ ohms = 12·1 ohms.

„ No. 2. $\frac{8 \times 1{\cdot}4}{1{\cdot}0}$ „ = 11·2 „

„ No. 3. $\frac{6 \times 1{\cdot}4}{1{\cdot}1}$ „ = 7·6 „

„ No. 4. $\frac{17 \times 1{\cdot}4}{0{\cdot}08}$ „ = 298·0 „

Inasmuch as the value of the term CR is always small compared with E, in the expression $e = E - CR$, a much greater error in the determination of R than that included in the above estimates will produce no material effect on the accuracy of the determination of e.

Voltameter No. 1.

Surface of each electrode = 27·5 square centimetres.

Mean Current, in microwebers.	Microwebers per square centim. of electrode surface.	Average value of E, in volts.	CR, in volts.	$e = E - CR$, in volts.	Remarks.
1·6	0·06	1·480	.....	1·480	No visible evolution of gas.
3·1	0·11	1·622	.....	1·622	„ „
8·5	0·31	1·878	.....	1·878	Distinct evolution of gas.
11·2	0·41	1·897	.....	1·897	„ „
25·1	0·91	1·971	.....	1·971	„ „
62·8	2·28	2·062	·001	2·061	„ „
119·4	4·34	2·129	·001	2·128	„ „
216·7	7·88	2·218	·003	2·215	„ „
310·0	11·3	2·274	·004	2·270	„ „
543·7	19·8	2·333	·007	2·326	„ „

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Voltameter No. 2.

Surface of each electrode = 8·0 square centimetres.

Mean Current, in microwebers.	Microwebers per square centim. of electrode surface.	Average value of E, in volts.	CR, in volts.	$e = E - CR$, in volts.	Remarks.
4·6	0·57	1·899	.....	1·899	No visible gas evolved
11·5	1·42	2·012	.....	2·012	Gas visibly evolved.
31·2	3·90	2·107	.....	2·107	,, ,,
52·0	6·55	2·170	·001	2·169	,, ,,
97·2	12·15	2·245	·001	2·244	,, ,,
141·6	17·7	2·302	·002	2·300	,, ,,
250·1	31·3	2·371	·003	2·368	,, ,,
508·0	63·5	2·468	·006	2·462	,, ,,
866·0	109·3	2·522	·010	2·512	,, ,,

Voltameter No. 3.

Surface of each electrode = 3·2 square centimetres.

0·12	0·03	0·892	.....	0·892	No gas evolved.
4·3	1·34	1·700	.....	1·700	,, ,,
6·4	2·00	1·996	.....	1·996	Slight evolution of gas.
10·3	3·2	2·086	.....	2·086	Gas visibly evolved.
12·8	4·0	2·153	.....	2·153	,, ,,
20·6	6·4	2·183	.....	2·183	,, ,,
29·0	9·1	2·231	.....	2·231	,, ,,
53·8	16·8	2·303	.....	2·303	,, ,,
99·0	30·9	2·368	·001	2·367	,, ,,
211·0	65·9	2·415	·002	2·413	,, ,,
384·0	120·0	2·468	·003	2·465	,, ,,

Voltameter No. 4.

Surface of each electrode = 0·19 square centimetre.

0·34	1·8	1·484	.....	1·484	No gas evolved.
0·9	4·7	2·032	.....	2·032	,, ,,
1·2	6·3	2·069	.....	2·069	,, ,,
2·8	14·7	2·275	·001	2·274	Slight evolution of gas.
3·4	17·8	2·291	·001	2·290	Distinct evolution of gas.
4·8	25·2	2·336	·001	2·335	,, ,,
6·1	32·1	2·367	·002	2·365	,, ,,
8·9	46·8	2·410	·003	2·407	,, ,,
16·6	87·4	2·496	·005	2·491	,, ,,
36·1	190·0	2·578	·011	2·567	,, ,,
67·1	353·0	2·629	·020	2·609	,, ,,
145·0	763·0	2·702	·044	2·658	,, ,,
248·0	1305·0	2·753	·074	2·679	,, ,,

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88. Figs. 2 and 3 represent the curves thus obtained, the curves numbered 1, 2, 3, and 4 being those respectively obtained with the voltameters thus numbered; in fig. 2 the currents are plotted as abscissæ, and the values of e as ordinates. Evidently, from the nature of these curves, the following conclusions may be drawn:—

(1) As the current increases with a given voltameter the value of e increases, but at a less rapid rate; so that the curves are concave downwards.

(2) For a given current the value of e is the greater the smaller the electrode surface.

In fig. 3 the abscissæ are the quantities of electricity flowing per second per square centimetre of electrode surface*; evidently the four curves obtained with the four voltameters do not differ more from one another than may reasonably be ascribed to the not absolute uniformity of the conditions of the surface of the electrodes (Nos. 1 and 2 being chiefly foil, No. 3 partly foil partly wire, and No. 4 wholly platinum wire), and to the unavoidable experimental errors; whence it may be concluded that

(3) for a given rate of flow of electricity per unit area of electrode, the value of e is independent of the size of the electrodes and of the current passing; *i. e.*, e remains the same when both electrode surface and current are altered in the same ratio.

Effect of Variation in the Strength of the dilute Acid used.

89. It has been concluded by previous experimenters that increasing or decreasing the strength of the dilute sulphuric acid electrolysed makes little or no difference in the value of e, and that, if any difference exist, it is in this direction—that the stronger acid gives, *cæteris paribus*, a smaller value for e, the effect being less nearly imperceptible with very weak solutions indeed, and being well marked when distilled water is compared with weak acid. The following experiments were made with the above-described voltameters, from which it results that as the strength of the acid is increased (from 0·2 to 40·0 grammes per 100 cubic centims.) a progressive diminution in the value of e ensues.

Acid containing 40·0 *grammes of* H_2SO_4 *per* 100 *cubic centims.*—Voltameter No. 1 was used: from Kohlrausch's numbers (Pogg. *Ann.* Ergbd. viii. p. 1, and clix. p. 233) it results

* Those determinations in which no hydrogen was evolved are omitted in every case, inasmuch as the effects of diffusion discharge are unequal for the different voltameters, and hence unequally reduce the value of e, leading to marked difference in the curves.

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that acid of 1·2207 sp. gr. containing 30 per cent. of H_2SO_4, and therefore containing in 100 cubic centims. 36·6 grammes of H_2SO_4, has a specific resistance of 1·37 ohm at 18°, whilst acid of sp. gr. 1·2625, containing 35 per cent., or 44·2 grammes per 100 cubic centims., has the specific resistance 1·40 ohm at 18°; whence evidently the specific resistance of acid of 40 grammes per 100 cubic centims. strength may be taken as 1·385 ohm at 18°, or close to 1·4 ohm, nearly the same as for acid of strength equal to 27·0 grammes per 100 cubic centims. (the minimum specific resistance being known to be at strength intermediate between 30 and 35 grammes per 100 cubic centims.); so that R is, as before, close to 12·1 ohms.

Current, in microwebers.	Microwebers per square centimetre of electrode surface.	E, in volts.	CR.	$e=E-CR$.
3·1	0·11	1·564	...	1·564
4·0	0·14	1·663	...	1·663
5·5	0·20	1·761	...	1·761
8·6	0·31	1·801	...	1·801
13·6	0·49	1·843	...	1·843
21·2	0·77	1·891	...	1·891
47·1	1·71	1·972	·001	1·971
76·3	2·77	2·034	·001	2·033
204·0	7·42	2·105	·002	2·103
281·0	10·2	2·143	·003	2·140
370·0	13·4	2·181	·005	2·176
679·0	24·7	2·223	·008	2·215
1140·0	41·4	2·303	·014	2·289

Acid containing 11·4 *grammes of* H_2SO_4 *per* 100 *cubic centims.*—Voltameter No. 3 employed. Value of R calculated from Kohlrausch's determination of sp. resistance* of sulphuric acid $=\frac{6\times 2\cdot 5}{1\cdot 1}=13\cdot 6$ ohms.

Current.	Microwebers per square centimetre.	E.	CR.	$e=E-CR$.
6·8	2·1	2·125	...	2·125
9·8	3·1	2·156	...	2·156
12·3	4·0	2·218	...	2·218
16·9	5·3	2·256	...	2·256
29·0	9·0	2·297	...	2·297
45·8	14·3	2·340	·001	2·339
70·1	21·9	2·362	·001	2·361
109·9	34·4	2·420	·002	2·418
212·0	66·2	2·486	·003	2·483
306·6	94·8	2·529	·004	2·525

* Acid of 1·0673 sp. gravity containing 10 per cent. H_2SO_4, corresponding to 10·7 grammes per 100 cubic centims., gives sp. resistance at

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By interpolation from these values it is rendered evident that whilst with acid of 40 grammes per 100 cubic centims. the values of e for given currents were uniformly lower than those for the same currents obtained with the same voltameter and acid of 27·0 grammes per 100 cubic centims., the reverse holds with acid of 11·4 grammes per 100 cubic centims. The following figures show that this is *à fortiori* true for acid of 1·0 and of 0·192 gramme per 100 cubic centims.

Acid containing 1·0 *gramme* H_2SO_4 *per* 100 *cubic centims.*—Voltameter No. 2 used. Value of specific resistance of acid of 1 per cent. or sensibly 1 gramme per 100 cubic centims., 22·1 (Kohlrausch), whence $R=\frac{8\times 22\cdot1}{1\cdot0}=177$ ohms.

Current.	Microwebers per square centimetre.	E.	CR.	$e=E-CR.$
3·7	0·46	1·939	·001	1·938
10·0	1·25	2·165	·002	2·163
36·0	4·5	2·285	·007	2·278
124·0	15·5	2·415	·022	2·393
291·0	36·4	2·495	·052	2·443
363·0	45·4	2·527	·064	2·463
498·0	62·2	2·607	·088	2·519
754·0	94·2	2·751	·133	2·618

Voltameter No. 1 used. Value of $R=\frac{13\times 22\cdot1}{1\cdot5}=192$ ohms.

Current.	Microwebers per square centimetre.	E.	CR.	$e=E-CR.$
7·8	0·28	1·993	·002	1·991
17·3	0·63	2·114	·003	2·111
27·2	0·99	2·141	·005	2·136
48·1	1·75	2·193	·009	2·184
88·7	3·23	2·253	·017	2·236
149·3	5·43	2·294	·029	2·265
324·3	11·8	2·387	·063	2·324
442·0	16·1	2·431	·085	2·346

Just as with acid of 27·0 grammes per 100 cubic centims. (§ 91 and 92), the two curves obtained with acid of 1 gramme per 100 cubic centims. with voltameters Nos. 1 and 2 are sensibly identical.

18°=2·59 ohms; and acid of sp. gr. 1·1036 containing 15 per cent., corresponding to 16·6 grammes per 100 cubic centims., gives sp. resistance 1·87 ohm; whence, by interpolation, acid of 11·4 grammes per 100 cubic centims. has sp. resistance 2·50 ohms.

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Acid containing 0·192 *gramme per* 100 *cubic centims.*—Kohlrausch and Nippoldt found that the resistance of acid of 0·2 per cent. H_2SO_4 was 465100 times that of mercury at 22°, whence the specific resistance of acid of ·192 gramme per 100 cubic centims. may be taken as close to 45 ohms; so that with the voltameter used $R=\frac{6\times45}{1\cdot1}=245$ ohms, the plates being 6 centimetres apart, and the bore of the tube between them averaging 1·1 square centimetre in section. The surface of each electrode with this voltameter was 3·5 square centimetres.

Current.	Microwebers per square centimetre.	E.	CR.	$e=E-CR$.
4·6	1·31	2·186	·001	2·185
15·2	4·34	2·300	·004	2·296
31·7	9·1	2·363	·008	2·351
60·3	17·2	2·468	·015	2·453
100·9	28·8	2·542	·025	2·517

Fig. 4 represents the curves obtained with these different strengths of acid, the abscissæ being in each case the current-flow per square centimetre of electrode surface: curve no. 1 is that obtained with acid of 40·0 grammes per 100 cubic centims.; the curve marked no. 2, that with acid of 11·4 grammes; those marked 3*a* and 3*b* the two with acid of 1·0 gramme per 100 cubic centims., obtained respectively with voltameters no. 2 and no 1; the curve marked 4 is that with acid of 0·192 gramme per 100 cubic centims.; whilst curve no. 5 is the average curve deduced for acid of 27·0 grammes per 100 cubic centims. from the preceding four sets of experiments. In all cases, *the weaker the acid the greater is the counter E.M.F. set up with a given rate of flow of electricity per unit area of electrode surface.*

[To be continued.]

XXXVII. *On the Figures of the Planets.* *By* Henry Hennessy, *F.R.S., Professor of Applied Mathematics in the Royal College of Science, Dublin* *.

IN the *Comptes Rendus* for June 14, 1880, I gave, for the compression of a planet from the action of superficial abrasion, the formula

$$e=\frac{5QD}{2(5D-3)-6(D'-1)},$$

* Translated from the *Comptes Rendus* of the Paris Academy of Sciences for January 31, 1881, p. 225.

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Q being the ratio of centrifugal force to gravity at its equator, D the mean density of the planet, and D′ the density at its surface. This expression may be more simply written

$$e=\frac{5}{2}Q\left(\frac{D}{5D-3D'}\right).$$

But, on the hypothesis of primitive fluidity, we have

$$e'=\frac{Q}{q}e_1,$$

where e_1 is the earth's polar compression, and q the ratio of centrifugal force to gravity at the equator. Hence

$$\frac{e}{e'}=\frac{5}{2}\frac{e_1}{q}\frac{D}{5D-3D'}.$$

For every planet in which the ratio of mean density to surface density can be assumed to be like this ratio for the earth, we shall have $\frac{D}{D'}=\frac{5.6}{2.6}$, and therefore

$$\frac{e}{e'}=\frac{70}{101}\frac{e_1}{q}=\frac{207}{303},$$

after substituting the values of e_1 and q. Thus, in such cases, the compression resulting from superficial abrasion would be sensibly less than that resulting from the hypothesis of primitive fluidity.

If we apply the formulæ to the planets whose times of rotation and mean density are most similar to those of the earth, we obtain some remarkable results.

For the planet Mercury, if we admit 86700″ for its time of rotation, ·075 for the ratio of its mass to that of the earth, and ·378 for the ratio of its diameter to the earth's mean diameter, we find $Q=\frac{1}{406\cdot3}$; and if the planet were homogeneous,

$$e=\frac{1}{325}.$$

With the same law of density as in the earth, on the fluid theory,

$$e=\frac{1}{413};$$

and on the theory of abrasion,

$$e=\frac{1}{586}.$$

These three results show that for Mercury no sensible compression is likely to be observed.

For Venus, if we adopt the values of the mass M, time of

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rotation T, and diameter a, generally admitted, namely

$$M=\frac{1}{412150},\quad T=23^h\ 21^m\ 22^s,\quad a=\cdot 954,$$

I find for the compression, on the hypothesis of fluidity and a law of density like that for the earth,

$$e=\frac{1}{247},$$

and by the hypothesis of abrasion at surface,

$$e=\frac{1}{351}.$$

The first of these values approaches closely to the compression recently observed by Colonel Tennant—namely, $e=\frac{1}{260}$. So far, therefore, the figure of Venus is more consistent with the theory of fluidity than with the theory of superficial abrasion.

Since I communicated my note on Mars to the Academy, I have become acquainted with the new determination of the planet's mass obtained from the motions of its satellites. The astronomers of the Washington Observatory have devoted especial attention to the satellites of this planet. Professor Asaph Hall has published results * which lead to the conclusion that the mass of Mars is probably about $\frac{1}{3093500}$.

With this value, and the values of other elements remaining the same as in my previous note, Q becomes $\frac{1}{203\cdot 74}$, or $\frac{1}{204}$ nearly. The compression on the fluid theory becomes $\frac{1}{206\cdot 96}$ or $\frac{1}{207}$. On the theory of abrasion the compression is $\frac{1}{303}$. The first is much nearer to the observed compression $\frac{1}{219}$ than the last.

It thus appears that, for the earth and the planets nearest to it, and whose mean density and general appearance make it probable that their materials resemble those of the earth in physical and mechanical properties, the compressions deduced from the theory of fluidity agree much better with observation than the compressions deduced from the theory of superficial abrasion.

* Washington Astronomical Observations, xxii., Appendix.

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XXXVIII. *Notes on the Construction of the Photophone.* *By* Professor SILVANUS P. THOMPSON*.

(1) IN the selenium photophone, light of varying intensity is received upon a prepared surface of sensitive crystalline selenium, the electric resistance of which it thereby changes. In the construction of the receiving "cell" it is obvious that certain relations must hold between the dimensions of the sensitive surface and the degree in which a given quantity of light will change the electric resistance—relations which ought to be observed in the construction of the instrument, and which are certainly worthy of investigation.

Professor Bell's typical selenium-cell consists of a small cylinder about 2 inches in diameter and 2½ inches in length (giving at most a superficial area of 15·8 square inches available), built up of alternate disks of brass and mica, filled between the edges of the brass disks with selenium, and having alternate brass disks connected up in multiple arc. This cell, in his usual apparatus, is placed at the bottom of a parabolic mirror.

Certain experimental observations made in attempting to repeat Prof. Bell's experiments led the writer to query whether this arrangement was the best possible one, and suggested an investigation, of which the following paragraphs are the chief points.

(2) THEOREM I.—*With a given maximum of incident light distributed uniformly over the surface, the change of electric resistance in a selenium-cell will vary proportionally with its linear dimensions, provided its parts be arranged so that on whatever scale constructed the normal resistance shall remain the same.*

Suppose there to be a cell of a certain size, having a certain normal resistance (*i. e.* a certain resistance in the dark as measured under a standard electromotive force), and presenting a certain area of surface; then, if a perfectly similar cell be made on a scale n times as great (in linear measure each way), the same total amount of light falling upon its surface will produce n times as great a variation in the electric resistance.

The proof of this theorem depends upon the law discovered by Professor W. G. Adams †—namely, that *the change in the resistance of selenium is directly as the square root of the illuminating-power.*

* Communicated by the Physical Society, having been read at the Meeting on January 22.

† Proc. Roy. Soc. vol. xxv. 1876, p. 113.

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For let it be supposed (as in the proviso of the theorem, introduced so as not to complicate the electrical conditions) that the enlargement should be to the scale $n : 1$ in all respects, save only in the depth of the selenium film, the brass conductors being the same in number as before, but of n times their former size, touching selenium along edges n times as long as before, the intervening selenium films being n times as broad as before. Such an enlargement will leave the normal electric resistance where it was before, provided the depth of the selenium films be not increased—though, as the photo-electric action is almost entirely a surface action, a slight increase in the depth of the film would probably produce no great change in its electric sensitiveness.

Suppose the light to be caused, by appropriate optic means, to fall upon the whole enlarged surface uniformly. The linear dimensions being increased in the ratio $n : 1$, the area will be increased as $n^2 : 1$. The average intensity of the illumination will now be $\frac{1}{n^2}$ of what it was. Each portion of surface equal to the original surface will receive but $\frac{1}{n^2}$ part of the whole light.

But by Adams's law the change of electric resistance is proportional to the square root of the illumination. Hence the electric effect over each portion of surface equal to the original surface will be $\frac{1}{n}$ of the original electric effect; and, since the effect is proportional also to the amount of surface which is under illumination, this quantity $\frac{1}{n}$ multiplied into the ratio of the enlarged surface to the former surface ($n^2 : 1$), gives for the total electric sensitiveness of the enlarged cell a value n times as great as that of the original cell. Thus the proposition is proved.

(3) Theorem II.—*With a given maximum of incident light the change of electric resistance will vary in proportion to the third power of the linear dimensions of the cell, if, while its linear dimensions are increased, the absolute thicknesses of the brass conductors and of the selenium films remain the same as before, and their number be proportionately increased.*

It was supposed above that the surface was increased n times by an enlargement in length and breadth, which left the total normal resistance where it was before. But since the breadth of the films is dictated solely by practical considerations of construction, the increase of linear width to n times will en-

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able n times as many conductors to be employed; and the thickness of the selenium film may be reduced to $\frac{1}{n}$ of what it was reckoned above. This will reduce the total normal resistance of the cell to $\frac{1}{n^2}$ of what it was reckoned above, and would therefore make it n^2 times as sensitive were its resistance the only one in the circuit.

Combining this result with the former, we obtain the result that the change of electric resistance exhibited by the cell of linear dimensions n, under the influence of a given quantity of light distributed uniformly over its surface, will be n^3 times as great as that exhibited by a cell of linear dimensions 1, provided that the absolute thickness of the films and conductors remain the same (the resistance of the brass conductors themselves being reckoned small).

(4) The practical inference from this is, that the selenium-cells should be made as large as possible, and that the beam of light received by the mirror from the distant station should be so constructed as not to concentrate the light on one point of the selenium, but to distribute it uniformly over the sensitive surface.

Now the supposed advantage of the parabolic mirrors hitherto employed is that they collect parallel rays to one focus. If this be no longer necessary or advisable, then some other form of mirror than that of the paraboloid of revolution ought to be employed.

(5) A short cone, polished on the interior surface, appears therefore to offer certain advantages over the paraboloid in respect of its distribution of light, besides being far cheaper to construct. It only remains to calculate the appropriate angle of aperture that shall, with a cylindrical selenium-cell of given length, give the greatest available linear aperture and reflect into the cell the greatest number of effective rays.

(6) Theorem III. *A hollow cone along whose axis lies a cylindrical selenium-cell of given length will reflect onto that cylindrical surface the greatest number of rays* (that traverse space parallel to the axis) *if its angular semi-aperture be* 45°.

The calculation amounts to finding the angle that will, with a given length of cell, give the greatest possible linear aperture.

In the figure 1, let POM represent the angle of half-aperture, which we will call θ. Let OQ ($=l$) be the length of cylinder, which may be supposed to be thin. Let the ray

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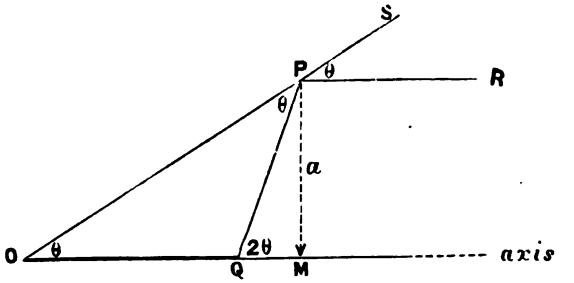
R P, P Q, be drawn making equal angles of incidence and reflexion with the surface of the cone. Then, since P R is parallel to the axis of the cone, the angle S P R=Q P O=θ, and the triangle O Q P is isosceles. Hence the exterior angle P Q M=2θ. If P M be drawn from P perpendicular to the axis, its length, which we may call a, will be that of the half-aperture.

Fig. 1.



Now $$\sin \text{P Q M} = \sin 2\theta = \frac{a}{\text{P Q}};$$

therefore $$a = \text{P Q} \sin 2\theta,$$

$$a = l \sin 2\theta.$$

Hence if l be constant, a will be a maximum when $\frac{da}{d\theta} = 0$.

Now $\frac{da}{d\theta} = l \cos 2\theta$; and equating to zero we find

$$\cos 2\theta = 0,$$

$$2\theta = 90°,$$

or $$\theta = 45°.$$

In other words, the mirror cone must have an apparent vertical angle of 90°, and its development will be a sector of 254° 33′·6 $\left(=\frac{360°}{\sqrt{2}}\right)$ cut from a circle whose radius is $\sqrt{2} \times$ O Q.

If the cylinder cell have itself a radius r, then the whole diameter of the linear aperture will be equal to 2 $(l+r)$; and the cone may be conveniently truncated at a distance along the axis from O equal to r, which would leave a circle of $2r$ diameter just fitting the posterior end of the cylinder.

It may be remarked that the anterior end of the cylinder will, when it is placed in position, be in the same plane as the

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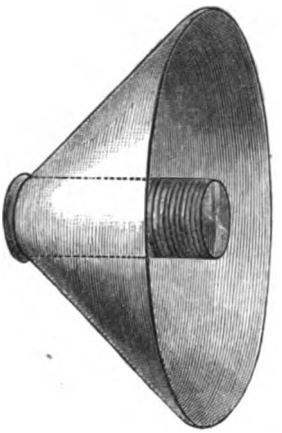
circular mouth of the mirror cone, and the general appearance of the mirror and cylinder will be that presented in figure 2.

Fig. 2.



With an angular aperture less than 90°, the depth of the mirror from back to front must be greater than the length of the cylinder; and the mirror, however prolonged, could not bring more rays to the surface of the cylinder except they underwent more than one reflexion.

If the angular aperture should be greater than 90°, the diameter of the cone that will reflect the effective rays will be less than that of the 90° cone, and hence cannot gather as much light.

One advantage possessed by such a mirror cone of the form specified above over any other form, parabolic or otherwise, is that all the rays meet the sensitive surface of the cylinder at normal incidence, and the loss by reflexion will be therefore a minimum.

(7) In preparing to repeat the Photophone experiments, the author has constructed sundry cells in a manner somewhat differing from that adopted by Professor Bell.

Finding it laborious to cut and fix the alternate disks of mica and brass, he constructed a cell by winding brass wires spirally round a glass tube so that the successive convolutions did not quite touch. Selenium was afterwards applied in the interstices, and alternate convolutions were connected metallically, the wires being cut and then soldered with alternate junctions. Afterwards two parallel wires, wound side by side as in a double-threaded screw, were employed. One of these cells was found by the author on Oct. 19th to have as small a resistance (in the dark) as 240 ohms. On account, however, of the wire not adhering firmly to the glass, and from other causes, the arrangement, though far more easily constructed than the built-up cell, was not satisfactory. Taking a hint from Mr. Shelford Bidwell, who has recently published a communication on the Photophone in 'Nature,' the author has constructed cylinders of slate grooved with a fine double-threaded screw, in which the parallel wires are laid. These cells prove much more satisfactory. Experi-

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ments are now proceeding with cells of this kind varying in length from two to eight inches.

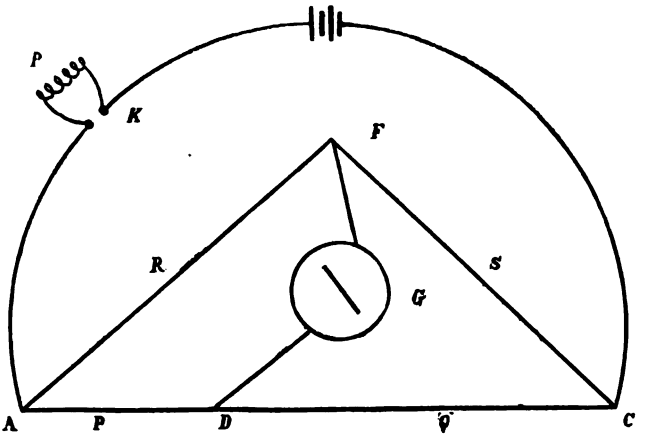
(8) The theorems enunciated above concerning the advantages of enlarging the size of the selenium-cell can readily be put to experimental test with cells such as described. There is of course a practical limit beyond which further increase of size will be of no advantage—such a limit being determined in particular cases by the resistance of the telephones and of the telephone-circuit, and by the other conditions, electrical and optical, of the experiment.

University College, Bristol,
Nov. 24, 1880.

XXXIX. *On the Measurement of Small Resistances.* *By* R. T. Glazebrook, *M.A., Fellow of Trinity College, and Demonstrator of Experimental Physics at the Cavendish Laboratory, Cambridge**.

WHILE measuring some small resistances with a Wheatstone's bridge at the Cavendish Laboratory, Mr. W. B. Allcock observed that the apparent measure of the resistance varied with the resistance in the battery circuit.

Let A D C be the bridge-wire, D being the point at which



the sliding contact is to be made. Let P be the resistance

* Communicated by the Physical Society, having been read at the Meeting on January 22, 1881.

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of A D, Q that of D C (P and Q include the resistances added to the ends of the bridge-wire), A F the resistance R to be measured, F C the standard S with which R is to be compared. At K in the battery circuit is a key.

In one position of the key the battery-current has to pass through the resistance ρ; in the other the ends of the coil ρ are connected, and it is thrown out of the circuit.

The experiment is as follows:—Place the key so that the resistance ρ is shunted, and adjust the position of D until there is no current through the galvanometer. Raise the key, and so throw ρ into the battery circuit; then the galvanometer-needle is considerably deflected, if the other resistances be small compared with ρ.

The experiment may of course be conducted in the reverse way, the position of equilibrium being found when ρ is in circuit. On depressing the key and shunting ρ the needle is deflected.

The deflection in this case is much greater than in the former; but then the battery-current is increased many times by the operation, and the apparatus therefore is rendered more sensitive; so that part of the effect may be due to a slight error in the original position of D.

The effect was reversed by changing the direction of the battery-current. This last fact showed that it could not spring from any heating of the coils. Moreover any heating-effect would be due to the difference in the temperature-coefficients of R and S or P and Q respectively; but R and S were throughout as nearly as possible equal and similar, as also were P and Q.

But it is well known that a thermoelectric effect is produced by the contact at D. This, as will shortly be shown, will explain the phenomenon; and the effect may be made a means of measuring, approximately at least, the electromotive force set up at the junction. For let E be the E.M.F. of the battery, e that at the junction; then we may show that the current through the galvanometer is

$$x = \frac{E(QR-SP)+e\{B(Q+R+S+P)+(P+Q)(S+R)\}}{D} \quad (1)$$

B being the resistance in the battery circuit, and D a function of the resistances with which we are not at present concerned.

The condition for no current, therefore, is

$$0 = E(QR-SP)+e\{B(Q+R+S+P)+(P+Q)(S+R)\},$$

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and in the case in which the resistances to be measured are very nearly equal we may put P=Q, R=S, in the coefficient of e, so that

$$\frac{P}{Q}=\frac{R}{S}+\frac{e}{E}\left\{2B\left(\frac{1}{R}+\frac{1}{P}\right)+4\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Thus if B is large compared with R and P, the term involving e is appreciable, and the usual condition P : Q = R : S is not sufficiently nearly true to give correct results.

The equation shows that the sign of the correction changes with that of E.

Let P′ and Q′ be the values of P and Q when the battery resistance is large.

Then

$$\frac{P'}{Q'}=\frac{P}{Q}+\frac{e}{E}\left\{2B\left(\frac{1}{R}+\frac{1}{P}\right)+4\right\},$$

and

$$e=\frac{E\left(\frac{P'}{Q'}-\frac{P}{Q}\right)}{2B\left(\frac{1}{R}+\frac{1}{P}\right)+4}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

With the view of testing the truth of this explanation, I made a series of experiments, using a wire bridge and various small resistances.

The resistance of the bridge-wire in the experiments was ·08 ohm; the wire was a metre long, and graduated to millimetres.

Different observations for the value of P agree to a fraction of a millimetre of the bridge-wire, while the differences in the values of P′ never amounted to more than 4 millimetres, and were usually much less. The value of ρ was 150 ohms. In some of the experiments Leclanché cells were used, in others Daniells. The E.M.F. of the Leclanché cells was found by the potentiometer method to be about 1·25 Daniell.

In the results given in the table, the E.M.F. of a Daniell cell is taken as 1 volt. The battery resistance is neglected compared with the 150 ohms interposed.

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Table giving Results of Experiments.

No. of cells.	Bridge-reading for P.	Bridge-reading for P'.	R in ohms.	Resistance added to arms of bridge.	e in volts.
	mm.	mm.			
3 Leclanchés ...	501 2	504·9	2·02	0	$75{\cdot}10^{-7}$
2 do., multiple arc	501·2	510·2	2·02	0	$60{\cdot}10^{-7}$
1 Leclanché.....	496·1	472·9	·04	0	$75{\cdot}10^{-7}$
1 Leclanché.....	544·6	526·3	2·02	2·2	$65{\cdot}10^{-7}$
1 Leclanché.....	705·5	644·0	2·02	5·2	$63{\cdot}10^{-7}$
1 Daniell	500·8	467·5	·02	0	$60{\cdot}10^{-7}$
1 Daniell	507·5	489·0	·04	0	$63{\cdot}10^{-7}$

Thus the mean value of e as given by the experiments with the Leclanché cells is $77{\cdot}6 \times 10^{-7}$ volt, while that as given by the Daniells is $61{\cdot}5 \times 10^{-7}$ volt. The wire of the bridge and the contact piece are platinum-iridium, the other wires are copper.

The closeness in the values of e would appear to show that the assumption of some action at this junction will account for most of the phenomena; while the constancy of the effect shows that, if due to a difference of temperature between the bridge-wire and the contact piece, that difference cannot vary much.

The hand of the experimenter was separated from the metal of the contact piece by a cap of wood. The effect of maintaining contact for some time after finding the equilibrium-position was tried without result. If the junctions A and C of the platinum-wire and the battery circuit be heated, the effect is decreased; if they be cooled, it is increased. Thus the effect, if thermoelectric, is due to heating at D.

In making measurements of resistance, this effect can of course be eliminated in various ways; it appears, however, to be worth while to call attention to its existence. One method frequently adopted to correct for this thermoelectric effect is given by Prof. Chrystal and Mr. Saunders (Report on the Standard Units of Resistance, B.A. Report 1876, p. 13). They made contact with the sliding piece before making the battery-contact, and waited until the thermoelectric current had attained a steady value and the galvanometer-needle come to rest in a position slightly disturbed from its original one. If on making the battery-contact no effect was produced, the resistance is given by the ordinary equation

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$P : Q = R : S$. This of course is only applicable when none of the coils used have any appreciable self-induction.

Another method is to reverse the battery-current.

Let P_1 Q_1 be the values of P Q when the current is in one direction, P_2 Q_2 when it is in the other. Then the true value of $\frac{R}{S}$ is

$$\tfrac{1}{2}\left(\frac{P_1}{Q_1}+\frac{P_2}{Q_2}\right).$$

A third method, applicable to coils with no self-induction, is to interchange the battery and the galvanometer. The thermo-electromotive force is then superposed on that of the battery, and the condition $PS = QR$ holds. The most serious objection to this seems to be that the whole current must pass through the sliding junction, which may thus get considerably heated, and so damage the bridge-wire. Perhaps the simplest correction is to keep the battery resistance low compared with the others.

I tried the effect of using copper wire for the bridge in order to have only one metal throughout; but I found that the contact between my fingers of two pieces of copper wire of somewhat different thickness produced a very appreciable current.

In the experiments given above the E.M.F. produced is about equal to that of a copper and platinum couple in which the difference of temperature is between 2° and 3°.

I was assisted in making the experiments by Mr. E. B. Sargant, Scholar of Trinity College.

XL. *Determination of the Density of Fluid Bismuth by means of the Oncosimeter.* *By* Professor W. Chandler Roberts, *F.R.S., and* Thomas Wrightson, *Memb. Inst. C.E.**

SOME time since, one of us described the results of experiments made to determine the density of metallic silver and of certain alloys of silver and copper when in a molten state†. The method adopted was that devised by Mr. R. Mallet‡; and the details were as follows:—A conical vessel of best thin Low-Moor plate (1 millim. thick), about 16 centims. in height, and having an internal volume of about 540 cubic centims., was weighed, first empty, and subsequently when

* Communicated by the Physical Society, having been read at the Meeting on February 12th.

† Roberts, Proc. Roy. Soc. vol. xxiii. p. 493.

‡ Proc. Roy. Soc. vol. xxii. p. 366, and vol. xxiii. p. 209.

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filled with distilled water at a known temperature. The necessary data were thus afforded for accurately determining its capacity at the temperature of the air. Molten silver was then poured into it, the temperature at the time of pouring being ascertained by the calorimetric method. The precautions, as regards filling, pointed out by Mr. Mallet, were adopted; and as soon as the metal was quite cold, the cone with its contents was again weighed.

Experiments were also made on the density of fluid bismuth; and two distinct determinations gave the following results:—

$$\left.\begin{matrix}10{\cdot}005\\10{\cdot}072\end{matrix}\right\}\ \text{Mean } 10{\cdot}039.$$

The invention of the oncosimeter* appeared to afford an opportunity for resuming the investigation on a new basis, more especially as the delicacy of the instrument had already been proved by experiments on a considerable scale for determining the density of fluid cast iron. The following is the principle on which this instrument acts.

If a spherical ball of any metal be plunged below the surface of a molten bath of the same or another metal, the cold ball will displace its own volume of molten metal. If the densities of the cold and molten metal be the same, there will be equilibrium, and no floating or sinking effect will be exhibited. If the density of the cold be greater than of the molten metal, there will be a sinking effect, and if less a floating effect, when first immersed. As the temperature of the submerged ball rises, the volume of the displaced liquid will increase or decrease according as the ball expands or contracts. In order to register these changes the ball is hung on a spiral spring, and the slightest change in buoyancy causes an elongation or contraction of this spring which can be read off on a scale of ounces, and is recorded by a pencil on a revolving drum. A diagram is thus traced out the ordinates of which represent increments of volume, or, in other words, of weight of fluid displaced—the zero-line, or line corresponding to a ball in a liquid of equal density, being previously traced out by revolving the drum without attaching the ball of metal itself to the spring, but with all other auxiliary attachments. By a simple adjustment the ball is kept constantly depressed to the same extent below the surface of the liquid; and the ordinate of this pencil-line, measuring from the line of equilibrium, thus gives an exact measure of the floating or sinking effect at every stage of temperature, from the cold solid to the state when the ball begins to melt.

* Wrightson, Journ. Iron and Steel Inst. No. II. 1879, p. 418.

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If the weight and specific gravity of the ball be taken when cold, we have, with the ordinate on the diagram at the moment of immersion, sufficient data for determining the density of the fluid metal; for

$$\frac{W}{W'} = \frac{D}{D'},$$

the volumes being equal. And, remembering that

$$W \text{ (weight of liquid)} = W' \text{ (weight of ball)} + x$$

(where x is always measured as a +ve or −ve floating effect), we have

$$D = \frac{D' \times (W' + x)}{W'}.$$

The following table shows the results of six experiments to determine the density of fluid bismuth made by the authors in the laboratory of the Royal Mint. The bismuth was kept just above its melting-point; and this was ensured by placing pieces of metal in the molten mass, which were observed just to melt.

No. of exp.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including this stem.	Floating effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
1.	2	23·33	9·72	1·0	10·13	Bismuth ball in fluid bismuth.
2.	2·25	33·46	9·755	1·3	10·11	do.
3.	do.	33·37	9·757	0·6	9·94	do.
4.	do.	33·53	9·774	0·7	9·98	do.
5.	do.	22·184	6·99 (iron)	9·3	9·92	Iron ball in fluid bismuth.
6.	do.	22·184	7·02 (do.)	10·2	10·25	do.
					Mean ..... 10·055	
					Specific gravity of solid bismuth ..... 9·82	

It will be seen that, considering the difficulties of manipu-

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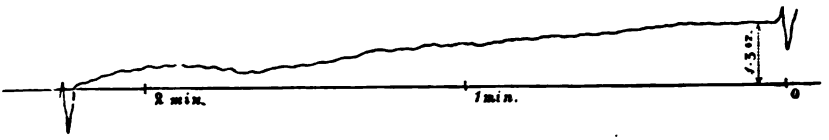
lation, the results are remarkably concordant, and their mean agrees very closely with that obtained by Mallet's method. We venture to think, therefore, that the density of bismuth in the solid and the fluid state may now be considered to be definitely settled.

Fig. 1 is the oncosimeter diagram of experiment No. 2 (see table), with a calculation of the fluid specific gravity annexed.

Fig. 1.

Oncosimeter Diagram of Bismuth Ball, 2·25 inches diameter, immersed in Fluid Bismuth (see table, Exp. 2).



Weight of ball and immersed part of stalk ..... 33·46 oz.
Specific gravity of do. do. ..... 9·755
First floating effect ..... 1·3 oz.

$$\text{Specific gravity of fluid} = \frac{34{\cdot}76 \times 9{\cdot}755}{33{\cdot}46} = 10{\cdot}11.$$

When first immersed, the floating effect is 1·3 troy ounce, which (with the weight and density of the ball known) is all we require to determine the fluid-density. Bismuth has a low heat-conducting power; and therefore the mass of the ball is reduced by surface-melting before much heat can penetrate to the centre. Hence the diagram does not accurately show, as in metals of high conducting-power, the change of volume, the effect being compounded with that produced by loss of mass.

In the case of iron, the conducting-power of which is high, diagrams taken with the oncosimeter show correctly the expansion of the ball until it is in a uniformly plastic state. Fig. 2 is a diagram of a cast-iron ball, 4 inches diameter, immersed in fluid iron of the same quality (No. 4 Cleveland). In this the solid iron is shown to be of less volume than the liquid for about 25 seconds, then to rise gradually in volume until in 4 minutes it becomes plastic (this having been proved by taking balls out in this stage of temperature, when an iron pin could be run through and through the metal as though it were a piece of putty). The slight fall of the line for about 2 minutes towards the left of the diagram probably shows a slight loss of mass owing to surface-melting; and then the whole ball melts with great rapidity, and as it joins the liquid metal of the bath the line of volume shoots rapidly down to

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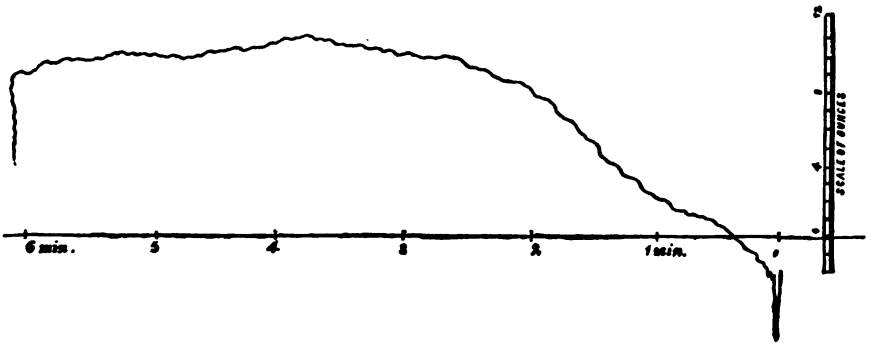
the equilibrium-line. This diagram shows, up to the plastic point (where loss of mass commences), the gradual change of volume at progressive intervals of time.

Fig. 2.

Experiment No. 23, on Iron.



Weight of ball and immersed part of stalk.....	132 oz.
Specific gravity of ball and immersed part of stalk	6·95
Maximum sinking effect.....	2 oz.
Maximum floating effect	11 oz.

Specific gravity of fluid iron $= \frac{6{\cdot}95 \times 130{\cdot}0}{132}$ ... $= 6{\cdot}84$

Specific gravity of plastic metal $= \frac{6{\cdot}95 \times 132}{145{\cdot}00}$ ... $= 6{\cdot}33$

4-inch ball of No. 4 foundry iron (Cleveland). Run with very hot metal. Immersed in No. 4 foundry iron.

The diagram read from left to right should represent the change from liquid to solid; and this is quite in accordance with other observations on cast iron*.

According to these experiments, iron expands rapidly (as much as 6 per cent.) in cooling from the liquid to the plastic state, and then contracts 7 per cent. to solidity; whereas bismuth appears to expand in cooling from the liquid to the solid state about 2·35 per cent.

* Journ. Iron and Steel Inst. No. II. 1879, and No. I. 1880.

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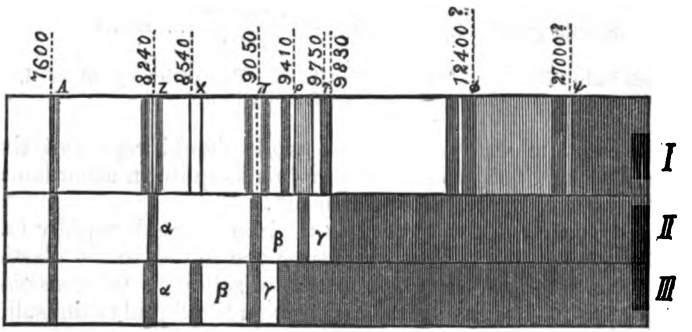
XLI. *On Lines in the Infra-red Region of the Solar Spectrum.* By Captain Abney, *R.E., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last number of the Philosophical Magazine Dr. J. W. Draper describes his phosphorographs of the solar spectrum, and incidentally brings my map of the infra-red region of the solar spectrum under review. From the date of his communication (Dec. 1, 1880), I am under the impression that his observations were based on a photograph of the map which I had the pleasure of sending him, and not after a perusal of the paper in the Phil. Trans. for 1880, a copy of which I also sent him more recently. Since Dr. Draper's remarks may cause a false impression in some respects, I wish to make a comment on one or two points which he has raised.

Dr. Draper says that the investigations which I have carried on "are not to be regarded as an extension of exploration in the infra-red region (for they really do not carry us beyond my own observations in 1843), but as securing the resolution of these lines* or bands into three constituent elements." In the paper already referred to, it will be seen that the knowledge of that region of the prismatic solar spectrum has been increased in several respects. The accompanying figure will show in some degree the extension. No. I. is the spectrum



of the infra-red region as measured from a photograph taken with three prisms. No. II. are the coincidences of α, β, γ as given by Dr. Draper in his communication. No. III. are the coincidences which I have obtained from reversed images. Up to W.L. 10,750 I have mapped with a grating; and taking Dr. Draper's coincidences of α, β, and γ as correct, instead of three lines or bands, we have some 280.

* α, β, γ, which Dr. Draper first showed to exist.

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Beyond these lines in the prismatic spectrum we have two other pairs of bands far away down, and approaching the theoretical limit of the least-refrangible end of the solar spectrum. The existence of these bands has, I believe, been surmised by Becquerel by observations with the thermopile, though I cannot at present find any published account of his experiments on this point.

The second point to which I wish to call attention is in regard to the cause of the reversal of photographic image. In a recent number of the Philosophical Magazine I gave a detailed account of some experiments made in connexion with the subject, showing that oxidation was the cause of most reversals, though in some instances it might be produced by a secondary action. The instance which Dr. Draper quotes (where Claudet destroyed a daguerrean image by red light, and was enabled to use the plate again) as showing that oxidation was not the cause of reversal, is in reality corroborative of my observations. In the 'Photographic Journal' for 1879 I have shown that where an image is destroyed by oxidation, whether by an oxidizing agent or by the action of light of low refrangibility, the plate is ready for a fresh exposure to the image formed in the camera; and I believe it is a common practice since that time for photographers to destroy fog (or the effect of light on a plate) by using an oxidizing solution. I may remark that the amount of oxidized matter is as small as that forming (what is called) the latent image itself, and is totally undistinguishable by any chemical analysis; and therefore it is not surprising that Claudet failed to find any alteration chemically in the sensitive film. I am not quite sure if I understand Dr. Draper to mean that W.L. 10,750 is beyond the theoretical limit of the spectrum. If he does, I think he will see that it is in reality when the W.L. is infinitely large—that is, when $\frac{1}{\lambda^2}=0$, according to Cauchy.

The plan of using phosphorescent material for viewing the infra-red rays I have frequently tried with success, and can quite confirm Dr. Draper's remarks. Mr. Warnerke was first in the field with these observations; and I left it to him to communicate them, which he has neglected to do. The use of phosphorescent material for any accurate measures is at present out of the question, owing to the explanation which the eminent American philosopher gives.

Yours faithfully,

W. DE W. ABNEY.

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XLII. *The Effect of Temperature upon the Electrical Resistance of Selenium.* By SHELFORD BIDWELL, *M.A.**

IT is well known that the electrical resistance of crystalline selenium is greatly affected by changes of temperature; but authorities differ in a remarkable manner as to whether a rise in temperature produces an increase or decrease of resistance. Some experiments which I have lately made may perhaps account for this discrepancy.

The temperature of the room being 14° Centigrade, a selenium-cell, made as described in 'Nature,' vol. xxiii. p. 58, was immersed in turpentine at 8° C. There was a great and sudden fall in the resistance. The temperature was then gradually raised. In passing from 8° to 24° the resistance steadily increased; from 24° upwards it rapidly diminished. For this cell, therefore, the resistance is greatest at 24° C. Five other cells were afterwards submitted to the same operation; and their resistance was found to be greatest at temperatures of 23°, 14°, 30°, 25°, and 22° respectively.

It should be noticed with regard to the cell whose temperature of maximum resistance is 14° (8° lower than any other), that it was prepared by very slowly *cooling* melted selenium from a temperature of about 250° C. Its surface is coarsely granular; its resistance is comparatively low; and it is not very sensitive to light. All my other cells were crystallized by gradually *heating* the vitreous selenium up to nearly 217° C. Their structure is very much finer; and though their resistance is higher, they are exceedingly sensitive to light.

The above results were obtained by observing galvanometer-deflections.

In order to form some idea of the actual amount of variation in resistance caused by extremes of heat or cold, an experiment was made with a cell whose resistance at ordinary temperatures in a dim light was about 110,000 ohms. When immersed in turpentine at −6° C. it was reduced to 49,000 ohms; and in paraffin at 115° C. it measured only 18,000 ohms.

All the experiments were made in a uniform light, which was only just strong enough to enable me to read the thermometer.

* Communicated by the Author.

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XLIII. *Notices respecting New Books.*

An Elementary Treatise on the Integral Calculus, containing Applications to Plane Curves and Surfaces, with numerous examples. By B. Williamson, F.R.S. Third Edition, revised and enlarged. London: Longmans, 1880.

WE used at Cambridge, "in the consulship of Plancus," to divide "articulate speaking" men into two classes—those who understood the Differential Calculus, and those who did not. Such a division still holds good; and possibly the relative magnitudes of the two divisions are still the same, though Todhunter has, since the epoch to which we refer, brought out his valuable books on the two calculi. An English student has still no great choice if he confines his reading to works in his own language. De Morgan's treatise remains still a vast repertory which repays occasional exploration; but who could take it as an ordinary text-book? Todhunter's works referred to above present the subject in a very clear light to students, and are enriched by a choice collection of exercises. Putting the four American text-books we have at hand (which are mainly founded on the English text-books and Bertrand's classical work) on one side, the only serious rival to Todhunter is the work before us. That its merits are of a very high order, and have been recognized by teachers and students, is evidenced by its having so soon reached a third edition. Its bibliography is as follows: in 1875 appeared a compact little work of 267 pages in nine chapters; in 1877 the work had grown to 348 pages, had developed a preface, and had increased to eleven chapters, the two new chapters treating of Moments of Inertia (a new subject in elementary treatises on the calculus) and Mean Value and Probability. For the former of these additions the author was mainly indebted to Prof. Townsend's papers in the 'Quarterly Journal of Mathematics;' and the latter was written by Prof. Crofton, whose exceedingly ingenious papers in the Phil. Trans. especially fitted him for this contribution. The third edition preserves the same order and number of chapters, but has incorporated numerous important additions, reaching to 371 pages, with a useful index at the end. These additions are all improvements; and so the work *vires acquirit eundo*. We shall not enter in any detail upon the contents and method of a work which has so rapidly risen into favour, but shall content ourselves with merely indicating one or two of the more important novelties. We commence with Frullani's theorem in Chapter vi. (on Definite Integrals): here Mr. Williamson refers to Mr. E. B. Elliott's contributions on the subject to the 'Educational Times,' and his extensions of formulæ, given in this chapter, to multiple definite integrals (Nos. 106 and 113 of the Proceedings of the London Mathematical Society), and to Mr. Leudesdorf's paper on the Extension of Frullani's Theorem (No. 131, Proceedings of the same Society). Some of these results have been inserted in the text. In the same chapter is an expression for the

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remainder in Lagrange's series in the form of a definite integral. This result, which seems to be due to M. Popoff, is given here in the way in which it is demonstrated by M. Zolotareff. The next point to which we would call attention is in connexion with the interesting subject of the areas of roulettes, to the study of which Steiner has contributed so many beautiful results. The additions are to what is now well known as Holditch's theorem, first published in the 'Lady's and Gentlemen's Diary' for 1858, in the appendix to which volume an elegant extension of the theorem was given by the Editor, Mr. Woolhouse. For the substance of the recent extension our author is again indebted to Mr. E. B. Elliott, whose paper appeared in the 'Messenger of Mathematics' for February 1878. This paper was fruitful in results, as it suggested further extensions by Mr. Leudesdorf and Mr. Kempe. In our text we have Kempe's "singularly elegant" theorem; but it strikes us that full justice has hardly been done to Mr. Kempe, or a part only of his results is given; this oversight can easily be remedied, should the author see fit, in his next issue*. The whole subject is just now occupying attention: we have noted a paper by M. Darboux in the *Bulletin des Sciences Mathématiques et Astronomiques*, and some short notes by Prof. Minchin. This latter gentlemen, we believe, is preparing an extended paper on the subject of roulettes; and Mr. Elliott is prosecuting his researches in the direction of kinematics on a sphere. But we have exceeded our limits, and must refrain from referring to the additions in the last two chapters. Mr. Williamson still excludes the "change of the variables in a multiple integral;" perhaps in a future edition he may say something upon this matter, and bring forward to notice the method employed by Mr. McColl (cf. his paper on the "Calculus of Equivalent Statements and Integration Limits," in the Proceedings of the London Mathematical Society, No. 124, and questions in the 'Educational Times'). The admirable manner in which the more ordinary parts of the subject, and notably the chapters on the geometrical applications, are handled thoroughly merits the approbation it has received; and we take our leave of Mr. Williamson, wishing his work *bon voyage* and himself "more power" still to write his promised (if we mistake not) treatise on Differential Equations.

XLIV. *Intelligence and Miscellaneous Articles.*

ON STEADY MOTION IN AN INCOMPRESSIBLE VISCOUS FLUID.

To the Editors of the Philosophical Magazine and Journal.

U.S. Coast and Geodetic Survey Office,
Washington, Feb. 15, 1881.

GENTLEMEN,

I FIND today in the February number of the Philosophical Magazine a letter from Dr. A. Oberbeck, of Halle, on the subject of my paper published in the November number of the Magazine,

* *Cf.* 'Nature,' vol. xviii. p. 148, June 6th, 1878.

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entitled "On Steady Motion in an Incompressible Viscous Fluid." Dr. Oberbeck calls attention to the fact that in my paper the same problem is proposed for solution that he investigated in vol. lxxxi. of Crelle's *Journal*. I obtained the number of Crelle referred to, and was struck by the similarity of the process in the latter part of my paper, which relates to the motion of a sphere in the fluid, and the method and results in Dr. Oberbeck's paper. For the method adopted, however, a reference to page 349 of my article will show that I did not claim absolute originality; I merely amplified a solution given by Mr. J. G. Butcher in vol viii. of the 'Proceedings' of the London Mathematical Society. The fact of having of necessity employed the methods of spherical harmonics, and also of having employed Borchardt's theorem, produces the only resemblance that exists in the two papers. As to Borchardt's theorem, I can merely say that I have used it a dozen times since I first became acquainted with it, in the winter of 1876, while attending a course of lectures on electricity in the Johns Hopkins University. I have used the theorem in three different papers on the motion of viscous fluids.

Dr. Oberbeck states that we propose the same problem. This does not appear to be the case, as a comparison of equations (4) of my article with his equations of motion will show. Dr. Oberbeck limits his problem to the case of very small velocities, such that the products of the velocities and their differential coefficients can be neglected. Such a restriction obviously does not obtain in my paper. The problem which I have proposed, therefore, and of which I have offered a *possible* solution, is much more general than that proposed by Dr. Oberbeck, and for which he obtains an *exact* solution. The vanishing of $\nabla^2\xi$, $\nabla^2\eta$, $\nabla^2\zeta$ has an entirely different significance in the two papers. In the form of the expressions obtained for the velocities in the case of a moving sphere, I cheerfully accord the priority to Dr. Oberbeck, though (from the different natures of the two problems) the quantities entering into these expressions do not have the same significance. I have only to say in conclusion that I have never seen Dr. Oberbeck's paper before today.

Hoping, gentlemen, that you may find it convenient to give this letter an early insertion in the Philosophical Magazine,

I remain, yours very respectfully,

THOMAS CRAIG.

DETERMINATION OF THE FUNDAMENTAL COLOUR-SENSATIONS BY THE STUDY OF THE DISTRIBUTION OF THE COMPLEMENTARY COLOURS IN THE CHROMATIC CIRCLE. (SECOND NOTE.) BY M. A. ROSENSTIEHL.

1. In studying the distribution of the red, yellow, green, and blue in the chromatic circle, I have found that there are in that circle three colours which, in relation to our eye, possess special

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properties; they possess the characters of the fundamental sensations of Young. I am about to demonstrate that they are also the only ones to which these qualities belong.

It is by no means sufficient, as Helmholtz says*, that when mixed in suitable proportions they produce the sensation of white†. Stated in these terms the problem is indeterminate; but from their definition itself two other properties are deduced which singularly contract the limits between which the choice can be made. (1) When mixed two by two, they produce all the intermediate colours (even those which, like purple, do not exist in the spectrum); (2) they fulfil this condition without at the same time producing the sensation of white—or, more exactly speaking, producing it less than any other colours which could be chosen in their place.

These two conditions are precisely the opposite of those fulfilled by the complementary colours, which on being mixed two by two do not give rise to any intermediate colour, and, when mixed in suitable proportions, produce only the sensation of white. By their properties these two sorts of colours reciprocally limit one another; hence it follows that the study of the distribution of the complementary colours in a chromatic circle permits the determination of the points of that circle which correspond to the fundamental sensations.

2. With the aid of rotating disks I have sought, for each colour, its complementary; the following table gives the result (the notation employed being that of M. Chevreul):—

Colours equidistant to the eye.	Complementary colours.	Ratio of the intervals‡.
Orange-red	5th green.....	6 : 1
Red	4th green.....	
Orange.....	Blue-green	6 : 1
Orange-yellow ..	1st blue-green	6 : 1
Yellow.....	Blue	6 : 5
Green-yellow ..	3rd violet-blue....	6 : 9
Green	4th violet.....	6 : 7
Blue-green	Orange.....	6 : 19
Blue.....	Yellow.....	6 : 12
Violet-blue	4th yellow	6 : 4
Violet	3rd green-yellow..	6 : 5
Violet-red	2nd green	6 : 5

3. At first sight great irregularity is evident in the distribution

* *Optique Physiologique*, p. 384, § 20.

† I do not say "white light," because lights of the same appearance may not be identical from the physical point of view: there are white lights of which the compositions differ, but the eye cannot distinguish them one from another.

‡ Between two consecutive colours of the first column of this table there are in reality intercalated five colours, designated by the numbers 1 to 5; so that six equidistant colours correspond to each *name*.

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of the complementary colours: the intervals are very variable (from 1 to 19); and it seems impossible to deduce from them any important consequences. Helmholtz* also, who specially studied their distribution in the spectrum, could not trace any law. This irregularity will disappear when the results are discussed, if we take as our guide the preceding definitions. The chromatic circle upon which my experiments were made consists of three sections, the limits of which are formed by the red, the yellow, and the blue. These three sections are not necessarily equidistant from one another; but the twenty-four colours of which each is formed fulfil that condition, as I have demonstrated in the first Note.

The following discussion, based on the assumption of equidistance, will not give rigorously exact results unless it be referred to a single section only. With this aim I select that which extends from the yellow to the blue, passing through the green, because in this portion of the chromatic circle the greatest number of complementary colours are assembled. The green, placed between the yellow and the blue, which by their mixture produce only the sensation of white, necessarily corresponds to a fundamental sensation. To fix its exact position (for we have the choice among eighteen colours of that name), I remark that with a second colour, at present unknown, but certainly situated between the red and the orange-yellow, and with a third colour, situated between the blue and the violet-blue, it must give the whole of the colours of the chromatic circle, at the same time producing the least possible sensation of white. It will therefore be situated at an equal distance from the complementaries of these two groups of colours.

At the first glance it might be believed that the question, being stated in such wide terms, would admit of several solutions. It is nothing of the kind. The complements of the nineteen colours extending from the red to the orange-yellow occupy in the circle only four consecutive numbers, or from the fourth green to the first blue-green. The complements of the blue to the violet-blue extend from the yellow to the fourth yellow. The colour which occupies the middle between the two systems is the *third or fourth green-yellow*. I cannot here give the process employed to determine the positions of the other two fundamental sensations; I confine myself to indicating the result.

The three colours are (to within $\frac{1}{12}$) the orange, the third green-yellow, and the third blue: they are equidistant from one another in the circle; and so are their complementaries—the first yellow, the blue-green, and the violet. The anomalies which I have pointed

* Helmholtz compared the wave-lengths of the complementary colours (*loc. cit.* p. 365), in order to discover the law of their distribution. He did not, and could not, arrive at any result. The quality possessed by two colours of being complementary is of a purely physiological order; while the wave-lengths (that is to say, the refrangibilities) are physical properties, which depend on the intimate nature of light: they would not cease to exist, even if we were deprived of an organ for their perception.

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out in the rough result of experiment thus disappear. The results obtained agree exactly with those recorded in my first Note. Maxwell*, who has studied the solar spectrum with the greatest care from the physiological point of view, has assigned slightly different positions to the colours which correspond to the fundamental sensations, as the following table shows:—

MAXWELL.

Place in the chromatic circle.	Place in the solar spectrum.
Third red	$\frac{1}{3}$ from C towards D.
Green	$\frac{1}{4}$ „ E „ F.
Fifth blue	$\frac{1}{3}$ „ F „ G.

ROSENSTIEHL.

Place in the chromatic circle.	Place in the solar spectrum.
Orange	$\frac{2}{3}$ from C towards D.
Third yellow-green	$\frac{2}{4}$ „ D „ E.
Third blue	$\frac{1}{3}$ „ F „ G.

As rotating disks permit us to experiment with greater precision, I believe these last data to be very near the truth†.

In brief, taking as a basis the equidistance of the colours which constitute each of the three sections of the chromatic circle, I arrive, by two analytical experimental methods, and without making any hypothesis, at proving that there exist three colours (the exact position of which I have determined) which possess relatively to our eye special properties. I have rigorously defined these properties, which coincide with those accorded by physiologists to the fundamental sensations. Thus the law of the mixture of colours, established *à priori* by Newton, developed by Young, Helmholtz, and Maxwell, is verified in its principles and specified in its consequences.—*Comptes Rendus de l'Académie des Sciences*, t. xcii. pp. 357–360 (Feb. 14, 1881).

ON TONES PRODUCED BY THE INTERMITTENT IRRADIATION OF A GAS. BY W. C. RÖNTGEN.

In my lectures on experimental physics I have for some time employed the following apparatus to render visible, in a simple way, the different capabilities of absorbing heat-rays possessed by the gases.

A glass tube about 4 centims. in diameter and 40 centims. in length, placed horizontally, is closed at both ends with plates of

* Proceedings of the Royal Society of London, vol. x. p. 404 (1860).

† Maxwell, moreover, operated upon only sixteen colours of the spectrum, which he mixed three by three.

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rock salt. In the middle between the two plates the tube is perforated in two diametrically opposite places. The upper opening communicates with a small glass tube that can be closed by a cock; the lower with a somewhat longer glass tube, descending vertically, which during the experiment dips in a vessel containing a coloured fluid. The fluid stands some centims. higher in the tube than in the vessel.

In front of one of the rock-salt plates, in a line with the glass tube, is a source of heat, say the gas-flame of an argand burner; between the flame and the tube a diaphragm about 4 centims. wide and a double screen of sheet metal are placed: the latter can be quickly drawn back and pushed forward.

Now the experiment is made in the following manner:—The height of the liquid in the manometer having been observed while the screen shut out the heat-radiation, the screen is quickly withdrawn; by the absorption of rays which now takes place on the part of the gas enclosed in the apparatus the gas is heated, in consequence of which the manometer shows a sudden increase of pressure, which, after some time, reaches a maximum. The increase, especially that which takes place at the first moment, is very different with different gases—comparatively slight with air, while it is considerable in the case of the strongly absorbing illuminating-gas and ammonia.

If the screen be then again pushed between the flame and the glass tube, the pressure diminishes, in correspondence with the cooling of the gas—at first quickly, afterwards slowly.

The phenomenon is, on the whole, tolerably complicated, because, besides the absorption-capacity, the specific heat also, as well as the ability of the gas to equalize more or less rapidly any differences of temperature that may be present, play a part; yet it is very suitable for a demonstration-experiment.

Now, after obtaining from M. Breguet's paper in the *Journal de Physique* for November 1880 a knowledge of some details of Mr. Graham Bell's experiments with the so-called photophone, the question arose in my mind whether the gas enclosed in the glass tube in the above-described experiment could not by intermittent irradiation be caused to emit sounds. The above-mentioned sudden rise and fall of the pressure at the moments when the irradiation commences and ceases respectively permitted a favourable answer to the question to appear possible; and experiment has, in fact, confirmed my supposition in a very satisfactory manner.

As the source of heat, I used Drummond's lime-light. By two lenses the rays were concentrated upon a notched disk of pasteboard, which could be rapidly rotated about a horizontal axis by means of a cord. In order to deaden as much as possible the noise arising from the rotation of the disk, it rotated between two larger fixed disks, which were provided with a notch corresponding to

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each of the openings in the rotating disk and closed by a thin plate of glass.

Behind these notches the absorption-apparatus was either fixed or held free in the hand; in this experiment it had a length of 12 centims.; the manometer was replaced by a short glass tube of 1 centim. diameter, over which was pushed a wide caoutchouc tube that led to the ear of the observer and was inserted in it as deeply as possible.

The rays penetrated into the absorption-apparatus every time an opening in the rotating disk came in front of the rock-salt plate; the interruption of this was effected by the unnotched parts of the disk.

At the commencement the apparatus was filled with air; on rotating the disk I could not perceive any tone, perhaps because, on account of the rotation &c., too many extraneous noises were still present. The affair, however, took quite another shape when illuminating-gas was substituted for air; the tone could be heard with extraordinary distinctness, and might almost be compared to the whistling of a not too strong wind. Its height varied with the velocity of the rotation; and the tone vanished only when the rotation became very rapid. The strength of the tone varied perceptibly with the time during which the cube was exposed; but the tones ceased directly if the rays were intercepted by an impervious body (as the hand, a small wooden board, or a hardgum plate) held before the disk.

With ammonia gas I likewise obtained distinct tones; while dry hydrogen and oxygen behaved like atmospheric air.

The explanation of these experiments is not far to seek; it has already been intimated above. We have not to do with any new property of the rays; the heating and expansion produced by absorption, and the subsequent cooling and contraction of the absorbing body, are the causes of the phenomena. That the gas really played the chief part in my experiments, and not the glass, upon which likewise the rays fell, follows even from this—that only the strongly absorbent gases emit audible tones; and I found direct proof of it by so directing the rays, in some experiments, by means of a third lens and a diaphragm, that they passed merely through the rock salt and the gas, without anywhere coming into contact with the glass wall of the tube: the effect was substantially the same as with the simple irradiation.

A solution of alum placed in the path of the rays caused an instantaneous cessation of the tone; on the contrary, scarcely a weakening could be observed when the rays had passed through a layer 10 centims. in thickness of solution of iodine (in sulphide of carbon). Consequently it is the less-refrangible rays which are most operative, at least upon illuminating-gas and ammonia.

I purpose investigating the behaviour of aqueous vapour, in the hope of furnishing in this way a contribution to the decision of the question whether it to any considerable extent absorbs heat-rays or

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not.—*Separate impression from the* XX. *Ber. der Oberh. Gesellsch. f. Natur- u. Heilkunde.*

Giessen, Dec. 8, 1880.

RESEARCHES ON THE SPECIFIC MAGNETISM OF OZONE.

BY M. HENRI BECQUEREL.

In the course of my researches on the magnetic rotatory powers of the gases*, oxygen presented some remarkable anomalies, which have induced me to resume the study of the magnetic properties of that gas, discovered by my father†, and to evaluate the specific magnetism of ozone.

For that purpose I fixed above the large electromagnet of the Museum a torsion balance enclosed in a vertical test-tube of glass, in which a vacuum can be produced and into which various gases can be introduced. The torsion-thread employed was a very fine gold wire 32 centims. in length, sustaining a small glass bar consisting of a tube filled with air, and closed at both ends. A microscope was directed towards a mark traced upon the bar, and permitted the latter to be at any time brought back to a fixed position, at about 45° from the line of the poles of the electromagnet.

When this was magnetized, the small bar was attracted; and by a suitable torsion of the gold wire the attraction was balanced, so as to restore the bar to its initial position. We know that in these circumstances, in virtue of a principle analogous to the principle of Archimedes, the attraction measured is the difference between the action exerted upon the bar and that exerted upon an equal volume of the ambiant gas. On measuring the attraction *in vacuo* we have the effect produced on the bar alone; and the difference between the torsion thus obtained and that which is observed in different gases measures the action exerted by the magnet upon the gas. It was moreover verified repeatedly, by determining the periods of the oscillations of the small bar under the influence only of the torsion of the thread, that the elasticity of the latter had not changed during the experiments.

The ozonized oxygen was prepared by passing pure and dry oxygen through an effluvia-apparatus like that prepared by M. Berthelot; and the gas was collected over sulphuric acid under a glass bell furnished with a glass cock. Thence it passed, through pipes entirely of glass, into the test-tube, in which a vacuum had been previously made.

Notwithstanding these precautions, perhaps by reason of the presence of traces of organic matters (grease, resin), the ozone in the test-tube was rapidly decomposed, and it was not possible to

* *Annales de Chimie et de Physique*, série 5, t. xxi. p. 289 (1880).
† Ibid. sér. 3, t. xxviii. p. 323 (1850).

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ascertain the precise composition of the ozonized mixtures to which the measurements correspond. The progressive augmentation of the attraction of the bar also permitted the decomposition of the ozone to be traced. The various analyses which I have been able to make, show only that the mixtures studied contained proportions of ozone which varied between 0·02 and 0·10 by volume. The results obtained are nevertheless sufficiently interesting, and may be, in the mean, summed up as follows:—

The small glass tube, when attracted *in vacuo*, was brought back to its original position by a torsion of 14° 22′. In oxygen at the temperature of 10° and pressure of 760 millims. of mercury, for the same magnetic intensity the torsion was not more than 3° 52′, which gives, for the effect produced upon oxygen alone, an attraction of 10° 30′.

Under the same experimental conditions, on substituting for oxygen a mixture ozonized by passing very slowly through the effluvia-apparatus at 10° temperature and atmospheric pressure, the torsion was 2° 29′; the attraction exercised upon the gaseous mixture was therefore represented by 11° 53′, or an increase of 0·13 above the specific magnetism of oxygen.

By placing the effluvia-apparatus in a refrigerating mixture of ice and sea-salt a gaseous mixture richer in ozone is obtained. This mixture, under the conditions of experiment which have just been specified, manifested an increase of 0·23, or nearly $\frac{1}{4}$, above the specific magnetism of oxygen.

Ozone is therefore more magnetic than oxygen; and, in spite of the uncertainty which exists respecting the actual composition of the ozonized mixtures investigated (an uncertainty which does not permit the precise number for ozone, supposing it isolated, to be given at the present moment), it is easy to see that the ratio of the specific magnetism of ozone to that of oxygen is *notably greater than the supposed ratio of their densities*. The specific magnetism of ozone is therefore greater than that which would correspond to the amount of oxygen it contains. This phenomenon is interesting in that it may be compared to those presented by certain magnetic bodies, which in different states of condensation give magnetic effects increasing much more rapidly than the ratio of the densities.

I am having an apparatus prepared, however, which I expect will permit me to ascertain the exact quantity of ozone formed at the moment of each experiment, and to give the true value of the specific magnetism of this remarkable body.—*Comptes Rendus de l'Académie des Sciences*, t. xcii. pp. 348–350 (Feb. 14, 1881).

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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MAY 1881.

XLV. *On a Proposal addressed to the Academy of Sciences of St. Petersburg, by* General Schubert, *relating to the Russo-Scandinavian arc. By* O. STRUVE. [Translation*.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN offering to you the accompanying translation of a paper which was first read nearly twenty years ago, I cannot but feel that some explanation is requisite. This will, it is true, be partly met at once by pointing out the authorship; but in addition to that, I may perhaps be allowed to give the assurance that the lapse of time has in no way diminished, but rather increased, its intrinsic importance. That the paper was originally somewhat controversial will not detract from its interest, especially as the issue will certainly be regarded by some as still undecided. For the rest, I think it desirable that the opinions of such high authorities as the authors should even now be made accessible to English readers, in their own tongue.

The science and practice of geodesy are alike confined in England to a small and, I fear, constantly diminishing, section of the community. But the question which is argued in this paper requires no very intimate acquaintance with geodetical technicalities or secrets, to enable the reader to understand what the main issue is; and there are few sensible arguments

* From the Bull. Acad. Sci. St. Pétersb. 1861, tom. iii. pp. 396–424. The original appeared in German.

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which have not an interest when that is the case. To those who are already cognizant of the issue, the arguments advanced by the authors on both sides cannot but have a stronger interest, especially if they have never seen, or have forgotten, the original; for that matter, I may say that I myself did not know of it, and have never seen an allusion to it in English literature.

I must not conclude this brief introduction without apology alike to authors and readers for the inelegance of the translation. A thankless task at best, translation becomes trying in the extreme when, as in this case, it is done first by a writer unacquainted with the technicalities of the subject, and afterwards revised. As I have dealt freely with the first *ébauche* in the earlier pages, and discarded it entirely in the later, I must take the whole responsibility, and will hope that at least some of the many blemishes will be forgiven.

I remain, gentlemen,
Very respectfully yours,
Collingwood, Feb. 1, 1881. J. Herschel.

On the 4th of May of the past year [1860] a paper by General Schubert was laid before the physico-mathematical class by the perpetual secretary, in which it was proposed, in reference to an essay published in the *Astronomische Nachrichten*, no. 1245, "Sur l'influence des attractions locales dans les operations géodésiques," that the Academy should undertake careful levelling in the vicinity of all, or at the least of some specially selected stations of the great Russo-Scandinavian Arc. According to General Schubert, these levels are necessary to bring the arc up to the present state of our knowledge, as without them it should not be considered complete.

At that time I had already started upon my journey to Spain for the purpose of there observing the eclipse of the sun. The Academy committed to Herr von Döllen, my representative for the time at the Observatory, the task of reporting upon General Schubert's proposal. Herr Döllen fulfilled the commission by an essay which was laid before the Class at its next sitting, on the 18th of May. His clear and lucidly expressed opinion proved to be almost throughout diametrically opposed to that of General Schubert, summing up the matter thus:—That he (Döllen) assented in no way, either to General Schubert's arguments, or to the proposals founded upon them. On account of the importance of the subject, and especially in consideration of the distinguished proposer of the scheme, who

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has for so many years occupied a position at the head of the extensive Geodetic Survey operations of the Imperial Staff, the Academy thought that Herr Döllen's report by itself should hardly be considered conclusive, and resolved to defer the decision of the matter until my return from abroad. At a meeting on the 19th of October the discussion was resumed; and I had then the opportunity of assuring the Academy that I quite agreed with Herr Döllen's opinion in every essential respect, and only differed from him somewhat as to the importance of certain details. After my remarks on these had been submitted to the Class, the matter again underwent minute discussion, as the result of which the Academy passed the resolution that no action need be taken on General Schubert's proposal, and that Herr Döllen's report and my subsequent remarks on it should be arranged in the form of an essay, in order to bring the grounds for this decision into general notice.

In executing this task we feel ourselves obliged to refrain from criticism upon the further contents of the above-mentioned paper of General Schubert's, as not directly appertaining to the subject, and will therefore confine ourselves simply to the consideration of the proposal in question.

Herr von Schubert expressly states that the Russo-Scandinavian arc, as it is now presented to the scientific world, might till a few years ago truly lay claim to the title of a completed work, and that it is only the appearance of the English book mentioned below* (marking, as it does, an era) that has materially altered the state of the case. The next thing, therefore, is to ascertain in what respect the state of the case is altered by the appearance of this work. It ranks unquestionably among the most important publications of our time in the region of geodetic literature. The care and circumspection evinced in this work, in the preparation of the data of observation which have been collected during the last three quarters of a century, and in the deduction of results as well for the English arc in particular as for the figure of the earth in general, fully entitle it to rank as a model.

As regards the arc, one material difference between this work and others is pointed out in the work itself, viz. (p. 742) that not, as is usual, has a selected triangulation, following as closely as possible the direction of the meridian, been alone considered, but the entire network of triangu-

* Ordnance Trigonometrical Survey of Great Britain and Ireland. Account of the Observations and Calculations of the principal Triangulation &c., drawn up by Captain A. R. Clarke, under the direction of Colonel H. James. London, 1858.

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lation which covers Great Britain and Ireland has been treated as one connected whole; and, after rigorous reduction, has served in its entirety for determining the distances between the parallels corresponding to the astronomical points. Now, although (as the author himself remarks) a close adherence of the triangulation to the meridian is not demanded for the determination of the parallels, in the present case, also, the great number of carefully determined azimuths and their agreement with one another, exclude any suspicion that inaccuracy is introduced into the work by the treatment actually adopted. On the other hand, the actual arrangement offers the very important advantage that a much greater number of accurately determined latitudes are thus brought into computation; whereby it happens that, while other arcs as a rule have only one latitude to each two degrees, here there are two or three on an average to each single degree. It is this great number of latitudes brought into the calculation which gives to the arc of the English meridian more importance, for the determination of the figure of the earth, than would be due to its mere extent in comparison with other measures; as thereby the possible influence both of local deflections of the plumb-line and of inevitable small errors in observation, upon the result to be obtained, is considerably reduced.

We find also, for our part, in the English work, a substantial gain in the fact that, in the determination of the figure of the earth by combining together all existing trustworthy arc-measures, those equations of condition are also brought in which express how much the deduced dimensions are affected by recognizing as subject to error the assumed relation of the standards employed in the different measurements. The actual magnitude of the errors of this kind can be discovered only by direct comparison of the different linear standards. This work of comparison has been performed with great care at Pulkova, for almost all known arcs. The table of the results of such direct comparisons, the completion of which, as given in the account of the Russo-Scandinavian arc, cost much time and trouble, and the importance of which has been recognized by their admission as aforesaid by the English geodesists, should certainly therefore be regarded as a distinct merit in the Russian work.

But Gen. Schubert considers the special merit of the English work to reside in something quite different from what we have mentioned. He regards it as a step in advance which marks an era, that the experiment has here been for the first time made of correcting each latitude, before it is used for deducing the figure of the earth, for the effect of the undoubtedly exist-

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ing influence of neighbouring surface-inequalities upon the direction of the plumb-line. We do not share this opinion with him: nay, we consider such correction for this purpose not even justifiable.

In order to present these differences of opinion in a clearer light, we must examine more closely the nature of the chief problem of geodesy—namely, the determination of the figure of the earth, and the effects of local attraction. At my instance Herr Döllen has undertaken this introductory presentation of the argument; and I think it best to give his own words, as his statements contain many thoughts which may be new to even experienced geodesists, and which certainly deserve consideration. At the conclusion of his exposition I will add a few remarks on my own account, in order to confirm what he has said.

Herr Döllen says:—"Let us for the present set aside the question of the linear dimensions of the earth; then the problem before us, of determining the figure of the earth, in the sense in which alone it is regarded in all researches of the higher geodesy, will be none other than this: To determine THE RELATION OF THE DIRECTION OF GRAVITY TO THE LOCALITY at any place upon the surface of the earth; or, in other words, to determine THE LAW ACCORDING TO WHICH THE DIRECTION OF GRAVITY VARIES WITH CHANGE OF POSITION ON THE SURFACE OF THE EARTH.

"It is essential to grasp this definition in its full significance, and especially to make clear to one's self, and to keep constantly before one's mind, what is the difference between the figure of the earth as we shall hereafter speak of it, and the figure of the earth as known in common parlance; remembering always that even in this common usage notice is not taken of those slight unevennesses (inappreciable as compared with the mass of the whole earth) which we call mountains and valleys. In order to grasp this difference fully, let us confine ourselves to the simplest conception, representing the earth as a perfect sphere, for instance, of a homogeneous but rigid mass; our arc-measurements would in that case present us with a sphere, as the figure of this earth, only if there were no rotation about an axis. Should this rotation exist, the figure of this earth, still actually spherical, would, according to the teaching of geodesy, be that of a spheroid or ellipsoid of revolution, with shortened axis. Alteration of the rate of the earth's rotation would alter only the proportion of the axes of this spheroid; any alteration, however, in the assumed uniformity or homogeneity of the mass in the interior of the earth might change the *geodetic* figure of the earth in endless ways, the exterior

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limits of the earth's body (we mean the *geometric* figure) still retaining, as before, the truly spherical form. It may appear, to say the least, inexpedient to use the term 'figure of the earth' in a sense so essentially different from that which has the best claim to be so called. This objection however disappears on closer enquiry. Such enquiry shows us that *in praxi* (that is, upon our earth as it is) this difference does not exist; or, to speak more correctly, does not exist for the greater part of it, because it is covered, at least over a very large portion of its surface, by a continuous fluid envelope. In this, the fluid portion of the surface, which we call the ocean, supposed at rest, the *geodetic* figure corresponds precisely with the *geometric* figure; for it is in the nature of a fluid that its surface must everywhere be perpendicular to the resultant of all the forces acting at each point. Such coincidence is certainly not necessary in the other, the solid, portion of the earth's surface; yet, from the fact that the mean height of land above sea, so far as our present knowledge extends, nowhere shows a relation to geographical position, we infer that here also the accordance of the geometric figure with the geodetic exists *as a matter of fact*—a position in the physical theory of the earth which, as is known, forms a starting-point for further arguments as to the condition of the earth's crust. It follows, therefore, that geodesy, which is of course limited, by the nature of its operations, to the solid portion of the earth's surface, is nevertheless in a position to determine the figure of the whole earth. In any case—that is to say, even supposing that in the future the figure of that solid portion should be recognized as quite different from that of the ocean—these geodetic results will still remain as an indispensable foundation for all further research.

"Our experience so far leaves us no longer any doubt that the figure of the earth, strictly considered, is an altogether irregular one. Should we now engage further in the attempt (surely a fitting one for geodesy to undertake) to know this irregular figure in all its particulars, then clearly we cannot avoid comparing the linear determinations on its surface individually with the corresponding angular celestial measures, by means of which the amount of change of gravity-direction becomes known. Any alteration whatsoever of one or other of the quantities so obtained by direct observation, would be plainly unreasonable.

"The scientific craving, however, after knowledge of law, on the one hand, and on the other, the practical need of a knowledge of the figure of the earth as a whole, as well as of those parts where no direct measurements exist, impose upon

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geodesy the further task of expressing as nearly as possible the whole surface of the earth accurately by a simple formula; that is, to find a formula which shall represent all observed quantities as nearly as possible; where it is scarcely necessary to add, that before the actual solution of the problem is entered upon, the expression here used, "as nearly as possible," must be assigned its strict mathematical significance. The figure expressed by this formula we will call, in opposition to the *actual* or *local* figure, the *mean* or *general* figure of the earth. The attempts which have been made to determine this figure permit it to be regarded, as things now stand, as one of revolution, the axis of which coincides with the earth's axis of rotation; for in the whole assembly of observations at our command there is not the slightest indication that any amendment of this assumption is demanded by *them**. The problem is thus essentially simplified, as it now merely consists in this, to find the equation to a curve by the rotation of which this *mean* figure of the earth shall be plainly presented. Now here, indeed, the question might be raised whether for this purpose, namely, the determination of the mean figure of the earth, it might not be proper to correct the observed astronomical amplitudes for the influence of visible inequalities of the earth's surface upon the direction of gravity at the points of observation. The question has here at least a totally different meaning from what it had above, where it had reference to the *actual* figure of the earth. This view, which is not the one we hold, is supported, as far as we can see, by the following arguments. Among the possibly manifold causes of the deviation of the *local* from the *mean* figure of the earth (for the future we will give this deviation the shortened title of *disturbance*) is to be included at all events the present one: we are in a position to determine its effect, or, say, a part of it, with some accuracy:

* That the contrary opinion has also been advanced from time to time might have passed unnoticed here, but that we wish to avoid any appearance of ignoring altogether General Schubert's recent paper on this subject ("Essai d'une détermination de la véritable figure de la terre") published by this Academy itself. Simply therefore for this reason, and certainly not for the purpose of offering criticism, which would here be out of place, we must say that the numbers given in that paper, if any importance can be attached to them at all, contain a testimony *for* and not *against* the statement made by us above. The whole essay seems to us to be a truly striking instance of what may happen, if one attempts to build upon figures derived from observation without first gaining a precise idea of the uncertainty which must attach to them on account of their nature and the manner of their acquisition. Positive conclusions are there based upon the *non*-agreement of numbers which, in truth, agree not only satisfactorily, but much more exactly than we could have expected.

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can it be doubted that, through the employment of the correction thus obtained, the directly observed latitude will be brought nearer to the mean latitude, and will become thereby more fitted for the determination of the mean figure of the earth? That the disturbing influence brought into the calculation in this manner is only a portion of the whole existing disturbance—and this for two reasons: first, because only the recognizable inequalities in the distribution of mass upon the surface of the earth, not the equally probable ones beneath it, can be taken into account; and secondly, because of these only such portion can be considered, as the actual extension of the levelling may have supplied data for—this is indeed to be regretted, but can be no reason for refusing to take account of it, even if it should perhaps prove probable or even certain that only a relatively *small* portion of the disturbance would thus ever be brought to our knowledge. The matter becomes plainer to the mind by an instance: suppose an astronomical station situated at the foot of the great pyramid of Cheops; let us say, in the middle of the southern side. We have not ourselves selected this station; we find it among the materials at our disposal, which we have to use to the best of our ability, for the determination of the figure of the earth. Can there be a doubt as to whether or not the correction of $0''{\cdot}7$ to the observed latitude at this station, which Peters has calculated for such a case, should be accepted, or whether a computer would be right who from prudence should entirely exclude this latitude? And if the correction is here obviously necessary, is the matter different if, instead of the pyramid, we think of Schehallien? or even, instead of this isolated mountain mass, of some more extensive inequality of the earth's surface, equally and, as regards its mass, as correctly known? In such a case we certainly cannot tell quite as well as in the case of an isolated mountain how far we should extend the calculation; this, however, makes no important difference. The answer to the question "how far?" is a decided one:—in every case, as far as possible; ten miles' distance is better than five; five is better than one; one better than none at all.

"In the foregoing remarks we have endeavoured to the best of our knowledge to bring forward and to place in the clearest light every thing that we can say in favour of the views which General Schubert maintains so warmly; and we acknowledge readily that a certain force in these arguments is not to be denied. If nevertheless, after mature deliberation, we must needs express ourselves as quite opposed to this opinion; in that we, in the present state of things, are not only far from considering as necessary the levellings recommended, but rather,

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supposing them completed, should regard their employment for the purpose of correcting the latitudes as injurious for the object of arc-measurement; then it certainly becomes our duty to justify such a difference of opinion by substantial reasons. We will proceed therefore to perform this duty, and hope that the arguments which are most important to the maintenance of our opinion will not fail in their effect upon the mind of the unprejudiced reader.

"Our opinion is this:—In all the preceding arguments one important circumstance is entirely left out of consideration, one assumption is constantly and tacitly made, which is indeed very necessary as affording these arguments some justification, but which yet in itself lacks all justification. The assumption is this—that between that portion of the whole disturbance which, as we said before, we are able to take into account, and that other portion which, for want of information we must disregard, there is no necessary connexion. If this assumption is well founded, if the whole disturbance is nothing but a lawless aggregate of fortuitous individual forces cooperating, then we cannot deny that the consideration of any one of them accessible to us would be a real gain; but if this assumption is not admissible, all inferences from it fall to the ground. That this really is the case (that is to say, that in judging of the admissibility or non-admissibility of the corrections, the assumption we have pointed out is the essential one) will be evident if we turn again to the examples just given. The true and only foundation of the absolute consent given to the use of the correction in the case of the pyramid consists in the fact that we know, in this case with certainty, that the disturbing mass considered is one that has been brought from a distance and placed there; so that there can be no question of any connexion between it and the surrounding masses on or under the surface of the earth. It is different, however, in both the other cases. Apart from the difficulties which are associated with the exact measurement of the geometrical form and solid content of the disturbing surface inequalities, the fullest confidence in the result of geological research must here be especially needed in order to repel the doubt whether certain counteracting effects must not necessarily be connected with the undeniable operation of these visible masses. This doubt seems all the more justifiable when one considers how comparatively insignificant are even the greatest depths at which any practical knowledge of the conformation of the earth's crust has been obtained; and when, further, it is remembered that even these few actual investigations can very seldom be undertaken at the actual spot, but that we must in

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such case allow ourselves to be satisfied with theories, the opinion will perhaps no longer seem strange that even in such cases as that of Schehallien, and all the more in those of wide-spread irregularities of uncertain constitution, the justification of the said correction has yet to be proved. No doubt about it exists of course in the mind of him upon whom lies the obligation of furnishing this proof. This is naturally the business of the one who asserts that the utility of the observed latitudes for the purpose of determining the figure of the earth is damaged by neglecting these corrections. Our wish, however, for as thorough an understanding as possible respecting a subject so full of importance induces us to examine it more closely, and to give our opinion as to the manner in which the desired proof might be satisfactorily conducted. To this end there seem to be three ways open to us :—First, there are certainly cases in which the origin of the existing surface-inequality can be declared with confidence, so far at least that not a doubt need remain that it has any connexion whatever with the distribution of density in the proximate interior. This presentment will receive confirmation by remembering the constant changes produced by water on the form of the earth's surface. The accumulations thus brought about by transference from distant and often extensive areas, are exactly analogous to the pyramid referred to above ; and with due regard to the circumstances, corrections to the latitudes of adjacent points may, in such cases, be deduced which, if these latitudes are to be employed for determining the mean figure of the earth, we may regard as fully justified. Secondly, we may be convinced, in some particular case, of the practical absence of a compensating effect in the interior of the earth ; and this may happen if we can prove the opposing or, more correctly the corresponding, disturbance of the latitude on the other side of the disturbing mass, let us say, of the mountain : it is to be remarked that this is always practicable, and presents no very great, and certainly comparatively few difficulties. When once the work has been performed which is necessary for determining the form and mass of the mountain, all that remains is the determination of a second latitude, and the trigonometric connexion of the two points of observation with each other and with the mountain. Then, if this condition is fulfilled—that the difference of the two observed latitudes is greater than it should be according to the geodetic distance, and that by just so much as the sum of the disturbing influences of the mountain upon the latitudes taken singly—it may well be admitted that on the whole the observed latitudes approach more closely through consideration of these influences

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to those which correspond with the mean figure of the earth, and are therefore also better fitted for the determination of this mean figure. It will be seen at once without special reminder that this is exactly the case of Schehallien; and it will be obvious that our willingness to allow the said correction to hold good in this case is in no way founded on any confidence in the assertions and deductions furnished by geology, but on the latitude on the other side of the mountain, and on the fact that, as the outcome of the whole investigation, a correct value has been found for the mean density of the earth; this is precisely what was demanded by the condition stated above, only in reverse order. Two further remarks, however, present themselves here to our mind, which seem to us sufficiently worthy of notice to permit a short interruption of the strict course of our argument. In speaking of the *correct* value of the density of the earth, we regard it as if it were known from other sources, whereas we are perfectly aware, not only that the determination of the same was the especial object of these very observations, but also that it was precisely these observations on Schehallien that gave the first conception of the earth's density. The fact is, however, that it is only the confirmation which this determination of the density of the earth has since received, by means of the decisive experiments with the torsion-balance, that enables us to acknowledge the force of any such proof—as will be seen when similar efforts made upon Chimborazo and Mimet are remembered. For it is clear that this method of determining the density of the earth lies open to the same objection which we make to the justification of correction of latitudes. Indeed, on closer examination we find that in this case the objection has even greater force; for as regards latitudes, something would at least be gained if only in general, that is, on an average of many cases, an improvement were effected by this procedure; whereas here, on the contrary, a determination of the density of the earth is supposed to be obtained in each case. But in fact nothing has been attained but some information as to whether or not the unknown disturbances of the direction of gravity affect the two points of observation differently; and it is clearly unreasonable to consider a widely aberrant result being due to such disturbances, and, on the other hand, to allow a closely accordant result to count as a true determination of the earth's density. Consequently, in one case as in the other, at Chimborazo as at Schehallien, these researches, for all that we can say at present, possess an exclusively geological interest; and to this we must add that even if a great number of *such* determinations were to hand, the whole of them could tell us nothing

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certain about the mean density of the earth; for there will always remain the well-founded suspicion of some necessary compensation. It is scarcely necessary to remark that all this refers equally to those researches in which the intensity, instead of the direction, of gravity has been chosen as the subject of observation. At the same time it must be granted that the danger of finding the observed difference disturbed by the action of foreign causes is smaller the nearer the stations of observation lie to one another. Accordingly we cannot quit this subject without having shown in few words how by a different procedure it might well be possible to set aside, if not altogether, at least in great measure, the above objection, and thereby to give to these observations an essentially different importance. In our opinion the common failure of all these researches lies in the fact that two stations of observation only have always been thought sufficient, whereas it is really by their frequency alone that a judgment can be formed as to whether the disturbing mass which has been calculated for is really all that has been effective*. The other remark to which we referred above, has a more direct bearing on the present subject. It seems that the second latitude, the determination of which we demand in order to justify a correction being made to the first on account of local attraction, is so essential that, while *without* it this correction has no foundation, *through* it, in the greater number of cases, all further researches become unnecessary. Let us examine somewhat more closely in this connexion the results of the investigation into the deflection of the plumb-line at Schehallien. It was found that on the north side this deflection amounted to $11''{\cdot}61 \times \frac{\rho}{\delta}$, and on the southern side to $9''{\cdot}17 \times \frac{\rho}{\delta}$; ρ and δ betokening the mean densities of the mountain and of the earth. Now, as the difference between the two observed latitudes proved to be $11''{\cdot}66$ greater than was demanded by the geodetic distance of the two points of observation, it was inferred that $20''{\cdot}78 \times \frac{\rho}{\delta} = 11''{\cdot}66$, or $\frac{\rho}{\delta} = 0{\cdot}561$; whence the deflection on the northern side would

* We must not ignore the fact that in the most recent determination of this kind—at Arthur's Seat, near Edinburgh—there are three stations of observation, one on the summit of the mountain, the other two on its southern and northern slopes, almost in the meridian of its summit. This seems to have been due, not so much to design as to the fortunate circumstance that there a trigonometrical station occupies the summit, and certainly not on account of any belief in what we consider to be the material importance of a third station; nor has its existence been taken account of in deducing the result.

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be 6″·52, and on the southern side 5″·14. We have signified above our assent to the employment of corrections to the observed latitudes corresponding to these deflections, viz. 6″·5 and 5″·1, should they be used to deduce the figure of the earth; relying on the fact that the value 4·7 resulting from these same numbers for the mean density of the earth agrees pretty closely with the value given by repeated and entirely independent experiments with the torsion balance; namely, 5·48 according to Cavendish, 5·48 and 5·58 by the two experiments of Reich, 5·66 according to Baily. Although it cannot be doubted that by the application of these corrections the individual latitudes are brought notably nearer to the mean, yet the figures themselves show that, even in this case, the accuracy so attained is only moderate. For if $\delta = 5{\cdot}5$ instead of the aforesaid 4·7, we have $\frac{\rho}{\delta} = 0{\cdot}48$, and therefore a deflection on the northern side 5″·6, on the southern side 4″·4; or together only 10″·0, instead of the observed amount 11″·66. The missing 1″·66 must be assigned (together with actual errors of observation) to disturbances unknown to us; and we have not the slightest reason to divide this amount between the two points in the same proportion as the attraction of the mountain, as we really do if we apply the corrections 6″·5 and 5″·1. Is it not clear now that we gain quite as much, and perhaps more, for the determination of the figure of the earth, by adhering firmly to the directly observed latitudes, without any corrections whatever? and will it not always be so, if only the two stations are so chosen as to have the disturbing mass, with the consideration of which we have to do, between them, so that they shall presumably be affected about equally and in opposite directions,—a condition for the fulfilment of which at most a general knowledge of the surroundings of the place will suffice? But it is needless to insist further on the practical importance of this consideration.

"Let us now turn back to the interrupted course of our argument to indicate the third and, to all appearance, the shortest and safest way, in which the said correction of latitude might be justified. It is by appeal to experience, that supreme judge in all matters relating to the study of nature. Could we succeed in obtaining an unmistakable decision from that tribunal, the matter would at once be set at rest. Indeed all the pains bestowed in the foregoing arguments to justify particular cases would be comparatively worthless, at any rate for the purpose in hand, could it be shown that by these corrections the observed latitudes are on the whole really assimilated to the mean ones. For in truth it seems to

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us that every objection would lose its force in presence of such a fact; nay, more, that thereby alone can a sure position be won, from which to form our conceptions of the origin and formation of the earth's crust, in place of the very vague suppositions and conjectures with which we have hitherto been obliged to be satisfied. Against this something can indeed be advanced; nevertheless we do not hesitate in the least degree to acknowledge the great importance that would attach to such a fact, were it well established. We think, however, that we are for that very reason doubly bound to allow no lack of proper caution in establishing it; first noting carefully in each case *all* the connected circumstances, and afterwards avoiding a general conclusion too hastily drawn from particular cases. For such a conclusion to be justified, it is not only necessary that it rest on a very considerable number of separate carefully verified experiments, but indispensable that these experiments have been made at points as widely distributed as possible over the whole surface. Yet when, with such restriction, we come to examine the sum-total of fact at present at our disposal upon this subject, the existing information proves to be insignificant, compared with what is required: indeed, no such scale is needed to show its utter inadequacy. But we must restrain the desire to give an account, in any degree correct, of the present state of facts in this important matter: it would be impossible without minute examination, the result of which would show most plainly that even the materials separately, apart from their scarcity, are for the most part far from sufficiently well-founded for any further conclusion to be safely drawn from them. Thus for example, for the cases from which, according to all likelihood, the most decisive indications would be to be looked for, namely the attraction of great chains of mountains, we have to depend almost exclusively on the results of the second Indian arc, the most northern stations of which are near enough to the Himalaya mountains for a very important deflection of the plumb-line to be expected. What is to be taught on this point by the Alps and the Pyrenees has not yet been examined sufficiently for us to be able to say more than that great deflections undoubtedly exist there. The same is true of the mountains at the Cape of Good Hope. We may also expect very valuable additions of information in future years from the Caucasus; but at present it is in the case of the Himalayas alone that anything determinate has been ascertained. Yet even here our present knowledge goes but a little way towards giving any really clear idea of the true

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state of the case. The subject has been investigated from different sides, so that we by this time possess a whole literature upon it. We must notice especially that it has been the occasion of a very thoughtful hypothesis by Airy, according to whom indeed, at a certain distance from them, the deflection corresponding to the apparent effect of these mountains should not be discernible. The matter, however, as we said before, is by no means exhausted, and this chiefly from the want of a greater number of astronomically determined points. For our purpose it will be quite sufficient to mention the following undisputed facts. At Kaliana, the most northern station of the prolonged Indian arc, in lat. 29° 30′ 48″, the attraction of the visible mass of the mountains lying to the north causes, according to Archdeacon Pratt's investigations, a deflection of the plumb-line of 27″·8; by so much therefore, if no other disturbance existed, must the actually observed latitude be less than the mean one, as deduced geodetically from far distant stations. Instead of this, in all statements of the mean figure of the earth as a whole, which may be considered at all admissible with respect to the body of existing measurements, we find for the latitude of Kaliana invariably a quite insignificant correction. Thus, in the comparison on pages 764 and 770 of the oft-cited English work, we find it to be not quite 2″; and the same thing is observable at stations lying farther away. It cannot be doubted, therefore, that there exist here, besides the obvious, calculable, and so to speak visible disturbances, others, invisible and counterbalancing, of nearly equal magnitude. Even if one does not believe in any special connexion or natural link between these two kinds of disturbance, but considers their undeniable existence side by side to be entirely accidental, still experience is in this case in no way in *favour* of the said correction of latitudes. We will not indeed say that it gives decisive evidence *against* it; for just as little as any one, on due consideration, will deny the natural connexion in this case, so are we, for our part, willing to imagine laws of formation and of stratification for these mighty far-stretching mountains so entirely different from those that prevailed at the origin of the immeasurably smaller and, so to say, wave-like inequalities of surface everywhere met with, as to disallow any reasoning from one to the other. As to these last, investigations have yet to be made. It has already been made obvious that we are very much in the dark about them. The only data of sufficient if not always equal trustworthiness that to our knowledge we possess are those given in the English work. Section xi. of

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that work contains a detailed account of the method of calculation employed; and the summing up on page 664 shows for fifteen places the amount of deflection of the plumb-line, in the direction of the meridian, caused by the inequalities of the surrounding country. But it is to be regretted that later, page 770, the comparison of the observed latitudes with those deduced from the combination of the various arcs is given for only nine of these places, and not for all the fifteen; which might have been done, even if all these latitudes were not used for the determination of the best mean figure of the earth. We were very near trying to remedy this difficiency, by which available material is so notably diminished, by the help of data elsewhere supplied in the work itself. On further reflection however we refrained from this undertaking, on the ground that, in a matter in which an intimate acquaintance with all the accompanying circumstances is essential, it is hazardous to go further than the author himself has done. We take it therefore to be the wisest way to repose upon what has been given in the work itself as the result of the investigations, especially as this result is arrived at so frequently and so harmoniously as we find it stated in different places in this work. The conclusions may be summed up in the following three propositions:—

1. If the observed latitudes are corrected according to the amount of disturbance shown by levelling, they still do not harmonize by any means entirely with those corresponding to the general figure of the earth; but divergences still remain which are much too great to be explained by unavoidable errors of observation.
2. But the agreement of the corrected latitudes is notably greater than that of the uncorrected.
3. On the other hand, the agreement will be none the greater the further the contours are extended.

"To understand this last proposition we must add, with reference to Airy's hypothesis above mentioned, that for the greater number of points which were contoured a two-fold value of the deflection of the plumb-line is deduced. These, which we may call A and B, are distinguished from each other, the levelling in the case of B being extended to much greater distances from the station than in the case of A. A is therefore only a part of B, representing what is due to that part of the mass nearest to the station. The third proposition says that A agrees more closely with the correction demanded by the general figure than B does.

"It is evident that such a fact, did it only rest on sufficient grounds, would have a decided bearing on the question before us.

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Through it direct evidence would be gained of the existence of some *natural* connection, for the possibility of which we contend, of the visible disturbances with each other or with unseen counteractions. The immediate consequence of this would be, that it would be necessary to decide in each case how far the contouring should be carried; for this question could no longer be answered as before, The further the better. Should it, however, be contended that the correction B does effect an improvement of the latitude (or, in other words, bring them nearer to the mean ones), we answer that, with the proof of correlation between the causes in operation, the whole affair puts on a different aspect—an assertion which we will examine presently. First, however, to guard against misapprehension of what is most certain, it must be said clearly that at present matters have not advanced nearly so far. Of the three propositions stated above, only the first has claim to general acceptance, and that indeed not since yesterday only; the other two depend upon so small a number of places that they can certainly not be regarded as established, even for the small area to which these places belong, namely Great Britain. Even were this the case, the area is yet so small in comparison with the surface of the earth, that a glance at the globe is sufficient to show the inapplicability of a conclusion drawn from such a portion to the whole. If, then, any force could be conceded to the evidence furnished by the English work, beyond the immediate range of the facts, it would tend rather to establish conformity of the disturbances in question to a law. But, in truth, nothing of the kind is yet proved; only the first noteworthy step has been taken on the road which will certainly lead us in time to a knowledge of wider truths in this region—a goal worthy indeed of the highest effort, but one the attainment of which is sufficiently difficult and uncertain without premature conclusions. In any case it is still far distant; nor till it shall really have been reached should we think of deciding the question as to the admissibility of these proposed corrections to observed latitudes for the purpose of a determination, however important, of the figure of the earth.

"In connection with what was said above as to the change of view which must follow upon proof of relation between the forces concerned, we would now show more distinctively how in such case a new conception of the figure of the earth will become necessary. In order that not the smallest doubt may remain on this important point, we will now at the close of our remarks recur to the simple illustration by which at starting we sought to show what is especially meant by geodetic

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figure: albeit the return to the beginning is really a step in advance, inasmuch as we see ourselves led forward through it to a new conception. Let us then once more conceive the earth as a homogeneous sphere: in consequence of its rotation, the geodetic figure of this ball becomes, as we said before, that of a particular spheroid: all observed amplitudes and measured geodetic distances thus come into perfect accord, or rather each of them can be exactly deduced from the other by calculation upon this spheroid. But now let us imagine that levellings have been extended over the whole surface of the earth. These would show us that only at the poles and at the equator would the surface be vertical to the direction of gravity. Every other point would be found to lie in a plane inclined towards the equator; towards which, were there water thereon, it would flow off. To fix our ideas, let us restrict our thoughts to the northern hemisphere: there we should at every point recognize an excess of matter to the north, and consequently should calculate for a deflection of the plumb-line towards the north, that is to say, for a positive correction to the observed latitudes. If we followed this up logically at every point, a figure would doubtless result, which would strictly represent all the data furnished by observation, namely the geodetic distances and the corrected amplitudes. Though it is unnecessary to consider at present this figure more closely, so much at least is evident, that it would be different from the one corresponding to the uncorrected latitudes. But now, what do we learn from this? It is an open choice which of the two we will have. Certainly it would be better to have both if possible; but can further argument be necessary to show the confusion that would be introduced into the whole matter should one allow oneself to be seduced into permitting small portions of these corrections to be made here and there!

"We think to stop at this point. To try and exhaust the subject would be neither suited to our powers nor demanded by the occasion. We have, on the contrary, tried to resist the frequently recurring temptation to closer detail or wider scope; but the object proposed we hope to have attained. Looking back upon the foregoing pages, we think to have shown that the view that, for the determination of the mean figure of the earth, directly observed latitudes may without further justification be corrected for the amount of the apparent disturbance of the plumb-line, and even that the calculation of these corrections is an essential requirement of an arc measure, rests either upon a confusion of ideas (that is to say, upon a want of comprehension of what is really needed), or upon sup-

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positions which are in themselves highly unlikely, and in any case entirely unproved. Such correction could be admissible only in the case where the disturbances are recognized as entirely accidental and independent. The decision as to whether they are so in general must be a matter of experience. If stress might be laid on experiments made up to the present day, they would decide against the view to which we object; but in fact they will bear no such strain. Meanwhile the purpose which these corrections are meant to serve, namely to raise the value of arc-measurements in determining the mean figure of the earth, may be attained with incomparably less trouble and to a much greater extent, in a perfectly unobjectionable way: we have merely to determine more latitudes. This might be gathered from what has been said; but it is clear of itself when it is remembered that we have to do with the mean figure of a body which is certainly irregular, which figure can only be ascertained by observations taken at as many points as possible.

"From among the foregoing remarks we think we may commend to further attention our objection to special determinations of the mean density of the earth, as well as the suggested expediency of an increase in the frequency of stations of observation.

"Finally, we must not neglect to defend ourselves expressly against the conclusion that because we regard the repeatedly mentioned levelling as of no avail for the purpose of aiding in the problem of the figure of the earth, we therefore think them altogether useless. So little is this the case, that we do not hesitate rather to count these and similar labours, and the solution of problems connected with them, amongst the most important features of the higher geodesy. It is only the application which it has been thought might be made, in this direction, of particular materials, and the opinion thence derived that such researches belong especially to the essentials of arc-measurement, which is erroneous. It would be going too far to attempt at present to describe in fuller detail the aims which, in our opinion, ought to be the principal ones; but we are glad, in reference to this question, to be able to give our entire and unqualified assent to a remark made by Gen. Schubert, which is, we think, a not unnecessary one—namely, that it would not be right to limit these researches to the disturbance of the plumb-line in the direction of the meridian, as has been done in the case of the English work before us."

To the foregoing examination by Döllen, and to the conclusions which he has founded upon it, I may signify my

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assent unreservedly and with full conviction. As regards the question now before us, of adjudicating upon the proposal of Gen. Schubert to the Academy, we are thereby led to this conclusion—that, so far as present knowledge goes, we can recognize no advance in the field of geodesy through the application of corrections to latitudes for calculable local disturbance, and that an unconditional employment of such corrections might lead to most erroneous conclusions. We therefore protest most decidedly against the opinion advanced by Gen. Schubert, that the Russo-Scandinavian arc is inconsistent with the present state of science because the ground in the vicinity of astronomical stations has not been minutely contoured.

In the next place, Döllen's argument sets forth clearly that the said contouring is of quite subordinate interest in the geodetic problem of determining the mean figure of the earth, and proves that greater frequency of astronomical determinations offers much more certain and efficacious means of rendering innocuous the influence of local disturbance of the plumb-line upon the desired result. On the other hand, however, let us not forget that the levellings accomplished in Great Britain around the astronomical points are of great interest, in that they represent fixed and certain standpoints in the course of geological inquiry. In this respect, we concede readily that the investigations into local attraction presented in the English work are to be regarded as marking an epoch. Had Gen. Schubert proposed that similar researches should be undertaken in Russia also, for the purpose of enlisting geodesy in the service of geology, in order to collect material for the latter, we should certainly not have refused to assent willingly to the proposition.

It may here indeed be mentioned that work of this description has been going on in Russia for years, and that under instructions from the Pulkowa observatory and with the special cooperation of its director. Regarding the work thus undertaken or planned in Russia I may perhaps be permitted to offer here a few words.

For the purpose of geology it is obviously not essential that the inquiry into local attraction should be prosecuted only at arc-stations. That this has been the case in Great Britain is simply due to the fact, that the whole country is there covered by the geodetic network, and that, from the way in which the work has been executed, all points which are precisely determined astronomically, and geodetically connected, have been incorporated into the arc-measure. It might indeed seem strange that these same researches have not been instituted for all, but only for some of the stations of the arc.

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This, however, may be explained, irrespective of the fact that what has been effected is only experimental, by considering that, notwithstanding all the labour and expense bestowed thereupon, the levelling operations in Great Britain have not yet progressed so far as to furnish such satisfactory data regarding the surface inequalities for all points as are available for those stations at which this work has actually been done. In Russia, on the other hand, as we know, the greater part of the triangulation is distinct from the arc. Any survey station which has been precisely determined astronomically would here serve the purpose for geological inquiry as well as an arc-station. The next question would then be, at which of these stations shall the inquiry be undertaken? In our opinion this question should be answered in this sense—that such points deserve to be worked as have an especially geological interest; points where already a marked local attraction is indicated, or where local peculiarities, such as mountain masses or depressions, promise evidence on particular doubts. In connection with this last category, Döllen has already alluded to the Caucasian survey. And, indeed, we may hope that by the prosecution under General Chodsko's direction of that triangulation, which, cutting the Caucasus nearly in the direction of the meridian, is intended to connect the Transcaucasian survey with that which has been executed in the southern part of European Russia, very valuable material will in the course of a few years be available for the confirmation or reversal of Airy's hypothetical explanation of the nearly inappreciable influence of the Himalayas upon the plumb-line.

Under the first category of points, namely such as are already indicated by remarkable local attraction, a prominent place must certainly be assigned in Russia to Moscow. From a large number of points, chiefly on the borders of the province of Moscow, connected independently of each other with the Moscow Observatory, the latitude of this last, gedoetically inferred, is found to differ on an average 10″ from the directly observed value. In a district apparently so little irregular as that which surrounds Moscow such a phenomenon is certainly to be regarded as startling. The magnitude of the deviation amounts to about four times the mean value of local attraction for any unspecified place on the earth's surface, as presented by the nearly coincident determinations, first, of Bessel, and more recently, of Clarke, in their works on the figure of the earth. Here, then, it behoves to make sure of the fact in the first place by the most precise measurements; then to trace the course of the anomaly in the neighbourhood; and then perhaps,

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by recourse to extended levelling combined with inquiry into the subsurface conditions, to reach some conclusion as to the extent to which the anomaly is represented by the configuration of the actual surface-strata. Under instructions from head-quarters, Prof. Schweizer, the director of the observatory at Moscow, has for several years been occupied with the first of these duties, and we may shortly expect a publication of the desired results. So far as we are aware, these operations, interesting as they are, do not progress, on account of insufficient means, with either the accuracy or the rapidity which their importance would seem to demand.

Another striking anomaly seemed a few years ago to be detected on the shores of the Ladoga Sea, at the town of Navoya-Ladoga, the longitude of which, deduced geodetically, differed notably from the astronomically determined value. The attention of the Pulkova Observatory was consequently directed to this point particularly, by reason of its neighbourhood. The difference, however, has been found much less by more recent determinations, which have shown that the former result was to a great extent due, partly to errors of observation, and partly to numerical errors in the determination of the longitude-difference. What still remains as the apparent local attraction at Novaya-Ladoga is nevertheless in excess of the average, being as much as 0·7 second of longitude, which in lat. 60° corresponds to 5 seconds of a great circle. It is not large enough, however, to demand a special investigation in this quarter.

Döllen has also brought clearly before us that reliable grounds for a decision as to the relation between local attraction and crust-conformation can only be acquired by levelling when it is of great extent. This is especially the case in Russia, where the irregularities are as a rule individually insignificant; the general elevation or depression of the land being, on the other hand, in certain directions very considerable. It follows that if we desire to make real progress in Russia in this direction, it becomes essential to determine the rise and fall of the land over as wide an extent as possible, by means of a system of connected levels. It is known to the Academy that the prosecution of such a general scheme of levels has been proposed by the Government to the chief Observatory, for other and more immediately practical reasons; and various preliminary inquiries have already been instituted with a view to ways and means to carry it into effect. Should this design, as we hope, be shortly put into execution, geology will certainly gain thereby a large body of material.

Before we bring to an end this our judgment upon the

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proposition of Gen. Schubert we think it undesirable to pass in complete silence over one point, which, though it does not belong directly to the subject, yet to avoid misunderstanding demands some explanation. We refer to the statement, frequently occurring both in General Schubert's essay in the *Astronomische Nachrichten* and in his communication to the Academy, that it is to Airy that the English arc owes its pre-eminent position as marking an epoch in geodesy, through the application by him of the before-mentioned corrections to individual latitudes. This statement seems to be entirely without foundation; for in the account of the English arc we find nothing which can be regarded as in favour of this statement, but rather the contrary. The only occasion on which Airy's name occurs in that work in connection with the investigations of local attraction is in the mention of his ingenious speculation by which he seeks to explain the phenomenon that the Himalayas exert no sensible influence upon the plumb-line at the neighbouring stations of the Indian arc. This speculation alone should have sufficed to prove that Airy did not approve of the application generally and unconditionally of such corrections. I have moreover had the opportunity, partly by letter and partly by oral communication, of learning what are Airy's views on this point; and think myself entitled to say that that distinguished philosopher is in agreement with me in the opinion that such correction of latitudes in general must be regarded as opposed to the geodetical purpose, while at the same time he certainly does not ignore the bearing which such investigations must have upon geological studies. If there is any thing in the said English work which could suggest the thought that Airy was directly concerned in it, it would be the careful, circumspect, and, in a word, masterly treatment of the geodetic material; but for the credit of this too, Airy, as I know from his own lips, waives all claim: it belongs exclusively to the authors named upon the title-page —to the present Director of the Ordnance Survey, Sir Henry James, and to his distinguished Assistant, Captain Clarke.

XLVI. *Certain Dimensional Properties of Matter in the Gaseous State. An Answer to* Mr. George Francis Fitzgerald. *By* Professor Osborne Reynolds, *F.R.S.**

IN the February number of the Philosophical Magazine there appeared a paper by Mr. Fitzgerald, in which he criticised my paper "On certain Dimensional Properties of Matter in the Gaseous State," Philosophical Transactions

* Communicated by the Author.

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of the Royal Society, 1879. Mr. Fitzgerald courteously put his remarks in the form of questions, expressing the hope that I would answer them. I was prevented by other work from preparing any thing in time for insertion in the April number; but I now ask your space for a few remarks.

The objections taken by Mr. Fitzgerald to my work may be summed up as three:—

(1) That by dividing space into eight regions I have adopted a method which is at once inelegant and unnecessarily elaborate.

(2) That I have omitted terms which, if retained, would have altered the results.

(3) That I have changed my views and adopted the theory which I had previously combated.

To all these accusations I would most emphatically plead *not guilty*. And I would further suggest, in explanation of Mr. Fitzgerald's difficulty, (1) that he has not paid equal attention to all parts of my paper, but has rather confined his attention to those parts which relate to the phenomena of impulsion, in which he seems to be especially interested, and that thus he has failed to see that, in order to obtain any results whatever for transpiration, the division of space into regions is necessary; and (2) that in his anxiety to find a different result in the case of impulsion from that which I had obtained, he has failed to perceive that the terms which I have neglected, and of which he instances one as disproving my conclusion, are of a distinctly smaller order of magnitude than those which appear in my result.

As regards, then, the charge of inelegance, I am sure that Mr. Fitzgerald would not for one moment have urged it had he not thought that the particular step to which he objects might be replaced by some other known method. One might as well abuse David because he used a stone and sling, as object to the inelegance of a mathematical method by which alone true results have been obtained. Of course I do not for one moment defend my method as being elegant, nor should I have noticed this remark were it not that, taken together with the more definite criticism to the same effect, it shows conclusively that Mr. Fitzgerald has failed to notice the gist of the greater portion of my paper—that he has failed to notice one of the most important terms in the equation of transpiration and the manner in which this term enters. In the paragraph beginning at the bottom of page 104 he says, "With the symbols and notation I have no fault to find; but I must enter a protest against his elaborate and totally unnecessary *division of space into eight regions*. He might have perfectly well calcu-

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lated equations (43) to (47) without rendering a difficult subject tenfold as elaborate as was necessary." And then he goes on to show how I might have obtained equations for the aggregate results at one integration. Clearly, then, he has seen no object in my division of space into regions, and is at a loss to account for it except as mere clumsiness in the integrations. Had he, however, looked closer, or even been careful to be accurate in his statement, he would have seen that the two equations (44), which are among those to which he refers, only apply to the partial groups for which u is respectively positive and negative, and that they contain a term which apparently disappears if the respective members of the two equations be added; and he would have seen that the same thing is true of equations (45)*, which hold only for groups for which v is respectively positive and negative, and from which two terms disappear when the results are added. Now these terms, which are the first and second, are sufficiently obvious in the partial equations, whereas they do not appear at all if the integration be extended to both groups; and if Mr. Fitzgerald had followed the next articles (83) and (84), he would have seen why these terms are important. To ignore these two articles is to ignore the method by which the results for transpiration are obtained; and these results were the main purpose of the preliminary work in the paper.

To obtain any results at all for transpiration, it is necessary to divide space into two regions, or else to consider the mean range s as function of the position of the point and discontinuous at the solid boundaries; and by the latter method the determination of the form of the function requires that space should be divided. The results depend entirely on the terms which, when s is constant, disappear in the complete integration, but which, if different arbitrary values are assigned to s for the different regions, do not cancel when the partial integrals are added. No result whatever is obtained by complete integration if s be constant; and although Mr. Fitzgerald does not seem to have noticed it, the late Professor Maxwell fol-

* The partial equations (45):—

$$\sigma_y^{v+}(\mathrm{M}u) = \frac{\rho\alpha \mathrm{U}}{2\sqrt{\pi}} - \frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha \mathrm{U}}{dy} - \frac{s}{2\pi}\frac{d\rho\alpha^2}{dx} - \frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha \mathrm{V}}{dx},$$

$$\sigma_y^{v-}(\mathrm{M}u) = -\frac{\rho\alpha \mathrm{U}}{2\sqrt{\pi}} - \frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha \mathrm{U}}{dy} + \frac{s}{2\pi}\frac{d\rho\alpha^2}{dx} - \frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha \mathrm{V}}{dx}.$$

The equation obtained by complete integration:—

$$\sigma_y(\mathrm{M}u) = -\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha \mathrm{U}}{dy} - \frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha \mathrm{V}}{dx}.$$

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lowed me in dividing space into two regions at the bounding surfaces, calling the two groups the *absorbed and evaporated gas*. But without the use of arbitrary coefficients he had no means of dealing with the variable condition of his gas, except by assuming that the same distribution holds in both groups at all points. To meet this assumption (which, he points out at the top of page 253*, is improbable) he had further to assume a highly complex and improbable condition of surface; and the result is that the equation he obtains (77) is short of the most important term. This term is that which gives the result when the tubes are small compared with s; and as this is the only case in which the results are appreciable, when Maxwell came to apply his equation to an actual case there was no sensible result.

In the first instance, I also began by considering space as divided only at the bounding surface, and, assuming the distribution in the two groups the same, integrated for the complete space; and the result I then obtained was precisely the same in form as that subsequently obtained by Maxwell. These results correspond with the experimental results for a tube whose diameter is large compared with s—called by Graham *transpiration*; but they do not at all correspond with the law which Graham found to hold when he used a fine graphite plug, and which I have shown to hold also with coarse stucco plugs when the gas is sufficiently rare, viz. that the times of transpiration of equal volumes of different gases are proportional to the square roots of the atomic weights. Graham had considered this law as depending on the fineness of the pores of the plug, and had suggested that the action then resembled that of effusion through a small aperture in a thin plate, rather than transpiration through a tube of uniform bore; and this is the assumption which Maxwell falls back upon to account for the difference between his calculated results and those of experiment. That I did not do the same was owing to my having, by reasoning *ab initio*, after the manner explained in the analogy of the batteries, in the very first instance found that the law of the square roots of the atomic weights must hold in a tube whenever the gas was so rare that the molecules ranged from side to side without encounter, and to my having proved by experiment that both laws might be obtained with the same plug by changing the density of the gas. It was thus clear to me that some term had been omitted in my equation; and after a long search it was found that, though the term vanished in the complete integral, it appeared in the partial integrals when space was divided into regions, and that, as the values

* "On Stresses in Rarefied Gases," Appendix, p. 249, Phil. Trans. 1879.

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of s were obviously different in the different regions, the assumptions on which the complete integral had been obtained were clearly at fault. The further division into eight regions was not only for the sake of symmetry, but that all the other terms which enter into the partial integrals might be examined, and as being necessary in particular cases—as, for instance, in that of a round tube, which is also treated of in the paper.

Having thus shown that, however elaborate and inelegant, the division of space into regions is essential, it is unnecessary to defend it on other grounds. But I may remark, by the way, that such a division does tend greatly to simplify the consideration of motion. This, I think, is proved by the universal adoption of north, east, south, west, zenith, and nadir.

I have dwelt at considerable length on the foregoing point, as the misconception of this point is fundamental to all Mr. Fitzgerald's criticism. The rest I may answer shortly.

With regard to Professor Maxwell's remarks on my paper, and his own work on the same problem, of course the sad circumstance of his death occurring, so that this was about the last work he did, renders it very difficult to approach the subject; but with reference to what I have already said, and in explanation of the apparently imperfect idea at which he arrived as to the scope and purpose of my method, it may be stated that, before writing his own paper, Professor Maxwell had only seen my paper in manuscript in the condition in which it was first sent in to the Royal Society, when the preliminary part was very much compressed, and, as I fear, somewhat vaguely stated, besides being founded on different assumptions from the present. Without entering further upon this now, I may refer to a letter which I addressed to Prof. Stokes after seeing an early copy of Prof. Maxwell's paper, and before I was aware of his illness, which letter was subsequently published in the Proceedings of the Royal Society for April 1880, p. 300.

Mr. Fitzgerald has asked me for an explanation of the system on which certain terms are retained and others neglected. This is difficult to give in a few words; but I was under the impression that it is sufficiently explained in the paper. It seems to me that the difficulty which Mr. Fitzgerald has found must have arisen from his having adopted the hitherto vague way of looking at the mean path of a particle (or in this case the mean range) as a small quantity, without strictly inquiring as compared with what it is small. In my paper, s is nowhere to be regarded as small except in cases where it comes into direct comparison with some definitely larger quantity. The small factors are $\frac{U}{\alpha}$, $\frac{s}{\alpha}\frac{d\alpha}{dx}$, and $\frac{s}{\alpha}\frac{dU}{dx}$;

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the squares of such quantities being consistently neglected. Such factors as $\frac{s^2}{a}\frac{d^2a}{dx^2}$ and variations of higher order are zero in the case of transpiration, but in the case of impulsion they are of the same order as the results. But the retention of such terms in equations (42) to (48), or in the fundamental theorem, would only give rise in the results to such terms as $\frac{s^3}{ra}\frac{d^2a}{dx^2}$; so that as long as s is small compared with r no error can have arisen from the neglect of these terms. And this is the only case to which these results have been applied, the extreme case where s is large compared with r having been dealt with by a special method which gives rigorous results. In the first instance, all terms of the second order such as $\frac{s^2}{a}\frac{d^2a}{dx^2}$ were retained; and it was only after it was found that these did not in any way affect the results as a first approximation that they were neglected. The terms I have neglected are, as far as I perceive, the same as those neglected by Professor Maxwell; and such was the care taken in this matter (which is of fundamental importance) that I am very confident that there is no mistake. On the other hand, it is difficult for me to see how Mr. Fitzgerald can have failed to see that the residual term, which he instances as showing that I am wrong in saying that my equations show that there is no force in the case of parallel flow, is distinctly of the second order of small quantities. But even to this term he has no right; for in order to obtain results to such an order the variations of s would have to be considered. It seems that Mr. Fitzgerald is of opinion that the parallel flow of heat does cause stresses in the gas, and that he has been trying to find that I have not disproved the possibility of such stresses. If he confines his attention to stresses of the same order of magnitude as those now shown to exist in the case of converging or diverging flow, he will find that both Professor Maxwell and I have proved the impossibility of their existence; but if he goes, as he appears unwittingly to have done, to a higher order of small quantities, then I have nothing to say, except that he has no inconsiderable task before him.

Lastly, as regards the charge of having changed my views and having adopted a theory which is practically the same as that which I had been previously combating, I can only say that against no theory have I said a word of which I do not maintain the truth. I have never asserted that the variation of pressure in the direction of the flow of heat, which I have consistently maintained to be necessary to the production of the phenomena of impulsion, may not be attended by a differ-

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ence of pressure in different directions; and, of course, I have known that such must be the case since the time that I have seen and proved by experiment that this direct variation of the pressure depends on the convergence of the lines of flow, which was before the letter referred to appeared in 'Nature.' But what I have consistently maintained is, that a difference of pressure in different directions (*i. e.* parallel and normal to the hot and cold surface) will not explain the experimental results; and this was the theory advanced in opposition to mine, and which Mr. Fitzgerald still seems inclined to defend.

I am asked to mention the result which is referred to in art. 54. I can only point to every phenomenon of the radiometer; for there the gas between the hot and cold surfaces always maintains a greater pressure on the hot than on the cold plate—a result which is fully explained in art. 129, as the consequence of the divergence of the lines of flow from the hot plate and their convergence onto the cold plate, shown in fig. 13. If Mr. Fitzgerald will only study the phenomena, he will see that it is he who has misapprehended the entire problem. He says a difference of pressure in different directions might tend to cause the plates to recede from each other. Obviously it would; but then there is not the slightest evidence that the plates do so tend to recede, while they actually move in the same direction, the cold plate following the hot. Hence no force merely causing them to separate can explain the phenomena. I have pointed this out over and over again, and now, so far from having changed my views, I have to go over the same ground again. I will take a simple case—a light mill with two equal radial vanes in the same plane, and on opposite sides of the pivot, one black and one white. Let the light be placed exactly opposite the vanes, and let the vanes be at rest. Also let the surface of the vessel and the gas be generally at the mean temperature of the vanes. If, then, the force were only such as tends to separate the hot and cold surfaces, there would be exactly the same force between the comparatively hot black vane and the colder glass as between the comparatively hotter glass and the colder white vane; for there are the same differences of temperature; and therefore the forces on the two vanes would tend to turn the mill in opposite directions, and the mill would remain at rest, instead of whirling round as it actually does. That the flow of heat caused the surfaces to follow each other was proved from the first by the experiments; and that there is no force causing the surfaces to separate of the same order of magnitude as the force which causes them to follow is now proved by the kinetic theory.

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I think that now Mr. Fitzgerald will reconsider his protest against § 53; for while maintaining, on the one hand, a theory fundamentally different from that in my paper, he can hardly maintain, on the other, that there are no such theories, and that they have not found supporters. But, in truth, the remark in art. 53 was not applied to the theory which Mr. Fitzgerald seems to be supporting; and as I am sure that he is not prepared to maintain that the phenomena of the radiometer take place in an *absolute vacuum*, or are due to the *same cause as gravitation*, I am sure that he will not wish to stand sponsor to all the thoories set forth since 1874.

In conclusion, I would say one word in acknowledgment of those remarks in Mr. Fitzgerald's paper that were the reverse of critical, and to confess that it is a matter of no small satisfaction to have found a reader of Mr. Fitzgerald's knowledge and acumen.

Owens College,
March 24, 1881.

XLVII. *An Integrating-Machine.*

By C. V. Boys, *Assoc. Royal School of Mines.**

[Plate VIII.]

ALL the integrating-machines hitherto made of which I can find any record may be classed under two heads:—one, of which Amsler's beautiful instrument is the sole representative, depending on the revolution of a disk which partly rolls and partly slides on the paper; the other, comprising all the remaining machines, depending on the varying diameters of the parts of a rolling system. As this subject has been treated so recently by Mr. Merrifield in his "Report on the Present State of Knowledge of the Application of Quadratures and Interpolation to Actual Data," read at the meeting of the British Association at Swansea, 1880, in which he briefly describes previous machines and refers to the papers in which a full description may be found, I do not think it advisable to say more concerning them, except that none of them do their work by the method of the mathematician, but in their own way. The machine, however, which I have the honour of bringing before the notice of the Physical Society is an exact mechanical translation of the mathematical method of integrating $y\,dx$, and thus forms a third type of instrument.

The mathematical rule may be described in words as follows:—Required the area between a curve, the axis of x, and two ordinates. It is necessary to draw a new curve such that

* Communicated by the Physical Society, having been read at the Meeting on February 26.

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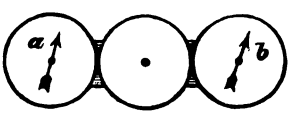
its steepness, as measured by the tangent of the inclination, for any value of x may be proportional to the ordinate of the given curve for the same value of x. The *ascent* then made by the new curve in passing from one ordinate to the other is a measure of the area required.

On Plate VIII. is a plan and side elevation of a model of the instrument made merely to test the idea: the arrangement of the details is not altogether convenient. The framework is a kind of T-square carrying a fixed centre B, which moves along the axis of x of the given curve; a rod passing always through B carries a pointer A, which is constrained to move in the vertical line $e\,e$ of the T-square; A then can be made to follow any given curve. The distance of B from the edge $e\,e$ is constant; call it k: therefore the inclination of the rod AB is such that its tangent is equal to the ordinate of the given curve divided by k; that is, the tangent of the inclination is proportional to the ordinate; therefore, as the instrument is moved over the paper, AB has always the inclination of the required curve.

The part of the instrument that draws the curve is a three-wheeled cart of lead whose front wheel F is mounted, not as a castor, but like the steering-wheel of a bicycle. When such a cart is moved, the front wheel F can only move in the direction of its own plane, whatever be the position of the cart; if, therefore, the cart is so moved that F is always in the line $e\,e$ and at the same time has its plane parallel to the rod A B, then F must necessarily describe the required curve; and if it is made to pass over a sheet of black tracing-paper, the required curve will be drawn.

The upper end of the T-square is raised above the paper, and forms a bridge, under which the cart travels. There is a longitudinal slot in this bridge, in which lies a horizontal wheel, carried by that part of the cart corresponding to the head of a bicycle; by this means the horizontal movement communicated to the front wheel of the cart by the bridge is equal to that of the pointer A; at the same time the cart is free to move vertically. It only remains to describe the mechanism which causes the plane of the front wheel of the cart to be always parallel to A B. For this purpose I make use of the principle of the epicyclic train. If three equal wheels are mounted on an arm, with their centres in a straight line and their edges in contact, any motion may be given to the arm or to the first wheel, a, yet lines on the first and last wheels, a and b, if ever parallel,

Fig. 1.



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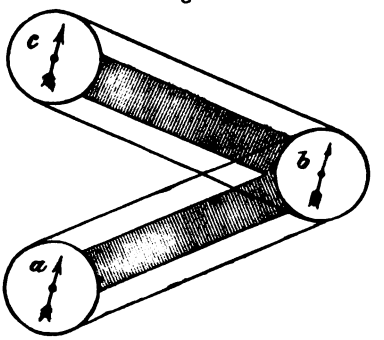
will always be so. Instead of the middle wheel an open band may connect the two outside wheels, with the same result. In the same way another arm may carry a third wheel, c, connected with b by an open band, as shown; then whatever motion is given to the wheel a or to the arms, lines on a and c, if ever parallel, will always be so. To apply this principle the wheel a is mounted on the fixed centre B, and its rotation is equal to that of the rod A B. A pair of arms hinged at H connect B with that part of the cart corresponding to the head of a bicycle, while the handles are replaced by the wheel c; the wheel b is mounted on the hinge H, and a single band goes round all three wheels, as shown. The wheel b and the arms are balanced about B by the counterpoise W. Thus, as A traces out any given curve, the front wheel of the cart has its plane always parallel to A B; and as the connexion between the cart and B in no way interferes with its vertical motion, the front wheel must describe the required curve. As the ascent in this curve is equal to $\frac{1}{k}\int y\,dx$, all that has to be done is to measure the ascent, multiply by k, and the product is the area required. If A is taken round any closed curve, the ascent can be measured immediately by a rule and multiplied by k as before.

Fig. 2.



In the instrument shown, k can be made either one, two, three, π, or $\frac{\pi}{2}$ inches. If the one-inch constant is used, the inclination of the rod A B is with large ordinates so great and the motion of the cart so nearly at right angles to the direction in which it is pushed, that there is danger of its being upset. This difficulty is in great measure avoided by inclining the board in the direction $e\,e$; and then, as the inclination of A B becomes greater and the power of the instrument to drive the cart becomes less, the action of gravity on the cart increases, and it can be moved with equal ease in all directions.

As the model works exceedingly well, I have no doubt that a carefully-made machine would give results as accurate as any other planimeter. As an aid in teaching physics to pupils not familiar with the principles of the integral calculus,

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and in illustrating those principles themselves, I think it would be found of very great value. To justify myself for bringing a subject purely mathematical before this Society, I will briefly give a few examples of its use. For simplicity's sake let $k=1$.

If A is moved along the axis of x (that is, $y=0$), the cart draws a horizontal line, the ascent is nothing, and the area is nothing. By this means any want of parallelism between the front wheel F and AB can be detected and set right (see Plate VIII. fig. 1).

If A is moved along a line parallel to the axis of x (that is, $y=c$), the cart draws the straight line $y=cx$; that is, the inclination is constant, showing that area is passed over uniformly (see Plate VIII. fig. 1).

If A is moved along an inclined straight line $y=cx$, the cart draws the parabola $y=\frac{cx^2}{2}$ (see Plate VIII. fig. 2). This is the path of a projectile: and the machine proves that it must be so; for taking abscissæ as time, the curve representing the velocity of falling is an inclined straight line, while the space fallen in any time, being measured by the area between the inclined line and the axis of x up to that point, is found by the cart; and as the horizontal movement is proportional to the time, the curve drawn by the cart is the path of a projectile.

If A is moved along the curve $y=\frac{1}{x^2}$, a curve representing attraction, the cart draws a rectangular hyperbola, showing that potential varies inversely as distance. As abscissæ are distances and ordinates forces, it is plain that the work done by an attracting body in bringing a unit from an infinite distance up to a point (that is, the potential at that point) is measured by the area between the curve, the axis of x, and the ordinate at that point; and as in finding this area the machine draws a rectangular hyperbola in which, of course, y varies inversely as x, it proves that potential varies inversely as distance (see Plate VIII. fig. 3).

If A is moved along the logarithmic curve $y=e^x$, the cart draws an identical curve; and this it should do, since $\frac{de^x}{dx}=e^x$ (see Plate VIII. fig. 4). Since the pointer A and the cart describe identical curves, it is plain that their distance asunder is constant; if, therefore, these two are connected by a link, and then the machine is started on the axis of x, they will each describe a horizontal line. But this will be an unstable motion; for if they depart ever so little from horizontal

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motion, they will turn aside faster and faster, the cart pulling the pointer and the pointer directing the cart, and thus originate the logarithmic curve.

If A is moved along a wave-line symmetrically placed with respect to the axis of x, the cart draws another wave-line a quarter of a wave-length behind the first in point of time. If the first line represents the varying strength of an induced electrical current, the second shows the nature of the primary that would give rise to such a current (see Plate VIII. fig. 5).

Fig. 6 shows the application of the machine to the determination of the area of a closed curve.

The rules for finding maxima and minima and points of inflexion are rendered obvious by manipulating the machine. By no means can the cart be made to trace a maximum or a minimum unless the pointer A *cross* the axis of x; nor can it pass a point of inflexion unless A pass a maximum or a minimum.

An indefinite integral requires the addition of a constant; but on integrating between limits this constant goes out. This is illustrated by the fact that the cart may be started on any level on the board, but the ascent made is the same.

Not only does the machine integrate $y\,dx$, but if the plane of the front wheel of the cart is set at right angles instead of parallel to AB, then the cart finds the integral of $\frac{dx}{y}$, and thus solves problems such, for instance, as the time occupied by a body in moving along a path when the law of the velocity at different parts is known. This is evidently true; for if a line be drawn perpendicularly to AB through B, it will cut $e\,e$ at a point distant from the axis of x by an amount equal to $\frac{1}{y}$, and therefore its inclination is such that its tangent is equal to $\frac{1}{y}$. As the cart travels down instead of up for positive values of y, its descent instead of its ascent must be taken.

Some modifications in the instrument would enable it to integrate y^2dx or y^3dx; it could also be made to integrate the product of two or more functions. I do not intend to go into details with regard to these extensions of the machine, but merely to explain the principle that would be employed. As before, let $k=1$. To integrate y^2dx the rod AB would be replaced by a T, as shown in fig. 3. The head of this would obviously cut the axis of x in advance of the edge $e\,e$ by an amount equal to y^2. Let a rod pass through this point of intersection and through a point on $e\,e$ distant from the

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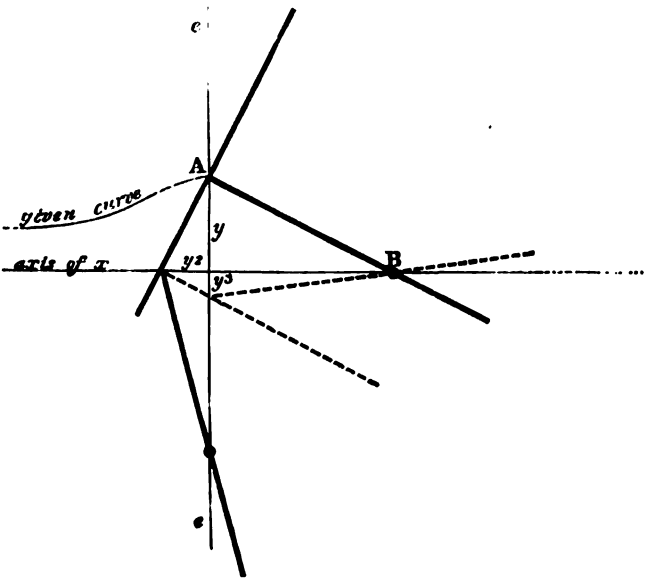
axis of x by an amount equal to k or 1, then the angle between this rod and $e\,e$ is such that its tangent is equal to y^2; and if the plane of the front wheel of the cart be kept at right angles to this rod, the cart will integrate y^2dx.

To integrate y^3dx the second rod would be kept parallel to A B, and the point where it cut $e\,e$ would be distant from the axis of x by an amount equal to y^3. If then the plane of the front wheel of the cart were kept parallel to the line connecting this point with B, as shown by the dotted

Fig. 3.



lines in fig. 3, the machine would integrate y^3dx. In these cases, as in the integration of products, it would be well to make k so large that the ordinates should never much exceed it.

To integrate the product of two functions—that is, to find $\int \phi x\, \psi x\, dx$, the two curves $y=\phi x$ and $y=\psi x$ would have to be drawn about two axes of x, one above the other, and two tracing-points, each on the line $e\,e$, would follow the curves. The fixed centre B would pass over the lower axis of x; but the epicyclic connexion, instead of joining B with the cart, would connect B with the upper tracing-point, and cause a rod passing through this point to be always at right angles to A B. This upper rod would cut the upper axis of x at a

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point distant from ee by an amount equal to $\phi x \psi x$; if, then, the front wheel of the cart is kept at right angles to a line joining this point of intersection with a point on ee distant from the upper axis of x by an amount equal to k or 1, then the cart will draw the line $y = \int_0^x \phi x \, \psi x \, dx$. The same principle might be applied to integrate the product of more than two functions.

As in the case of simple functions, so with squares, cubes, and products, the reciprocal could be integrated by twisting the plane of the front wheel of the cart through a right angle. Also the integral of $\frac{\phi x}{\psi x}$ could be found by making the lower curve $y = \psi x$, the upper curve $y = \phi x$, and by keeping the rod which passes through the upper tracing-point parallel instead of at right angles to A B.

The axis of x is drawn by using a little T square of such a length that its end is the same distance from the edge of the board as the fixed centre B; then a pencil held at the end while the square is moved across the board will at once give the axis of x.

I do not consider the modifications of the machine to be of much importance; but the simple machine as described in the first part of this paper, is, I think, interesting as being the only one that really illustrates the mathematical formula for the integration of $y\,dx$.

My thanks are due to Mr. Hilger, of Tottenham Court Road, for the great care and skill which he has bestowed upon the construction of the instrument. Certain modifications of detail, however, have been introduced which make it more easy of manipulation without sacrifice of simplicity.

Physical Laboratory,
South Kensington.

XLVIII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part IV. *By* C. R. Alder Wright, *D.Sc. (Lond.), Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School.*

[Concluded from p. 283.]

Effect of Variation of the Material of the Electrode.

90. It is well known, from the experiments of previous observers, that the nature of the surface of the electrode considerably affects the value of the counter E.M.F. set up, and that in particular platinizing a platinum electrode largely decreases the counter E.M.F. set up by a given current.

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This effect, however, is partly due to the considerable increase in effective surface thereby occasioned. In order to see what effects would be produced by substituting polished gold or compact carbon for polished platinum, the following experiments were made:—The carbon electrodes were thin slabs about 1–2 millimetres thick, sawn from hard rods used for electric lights; they were were filed to a tolerably smooth surface, and digested for some months with aqua regia—also boiled at intervals with hydrochloric, nitric, and dilute sulphuric acids. To make connection with the battery, the upper portions were wrapped round with platinum foil tightly tied on with platinum wire, connected to the glass mercury-cup supporting the electrodes; the whole of the platinum foil and wire was then covered with gutta percha, so that only carbon was exposed to the liquid in the voltameter. To make sure that as little as possible of metals &c. capable of being acted on by nascent electrolytic oxygen was present, they were used as + electrodes for several days in the electrolysis of dilute H_2SO_4, such a current being employed as to keep a continuous evolution of gas at the rate of some 50 to 100 cubic centims. of hydrogen per day. The superficial area of each electrode (measured by scale and compasses, and making no allowance for increase of surface due to the carbon being slightly pitted and not smooth) was 3·9 square centimetres.

The gold electrodes were similarly mounted, a platinum wire being fused to them, and the surface of the platinum covered with gutta percha, so that only gold was exposed to the liquid in the voltameter. Surface of each electrode = 17·0 square centimetres.

Values obtained with Acid containing 27 grammes per 100 cubic centims.

Gold Electrodes.

The distance apart of the centres of the electrodes was 15 centimetres, and the bore of the tube between them 1·1 square centimetre; so that $R = \frac{15 \times 1{\cdot}4}{1{\cdot}1} = 19{\cdot}1$ ohms.

Current, in microwebers.	Microwebers per square centimetre of electrode surface.	E, in volts.	CR.	$e = E - CR$.
9·8	0·58	2·139	...	2·139
14·7	0·86	2·312	...	2·312
34·0	2·00	2·407	·001	2·406
55·7	3·27	2·452	·001	2·451
156·9	9·23	2·515	·003	2·512
364·0	21·4	2·627	·007	2·620
668·6	39·3	2·701	·013	2·688

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Carbon Electrodes.

The distance apart of the centres of the electrodes was 18 centimetres, and the bore of the tube 1·5 square centimetre; so that $R=\frac{18\times 1\cdot 4}{1\cdot 5}=16\cdot 8$ ohms.

Current, in microwebers.	Microwebers per square centimetre of electrode surface.	E, in volts.	CR.	e=E−CR.
6·7	1·7	1·446	...	1·446
8·1	2·1	1·460	...	1·460
10·9	2·7	1·504	...	1·504
16·7	4·3	1·583	...	1·583
32·2	8·3	1·662	·001	1·661
103·3	26·5	1·761	·002	1·759
135·8	34·8	1·782	·002	1·780
238·0	61·0	1·829	·004	1·825

Values obtained with Acid containing 1 *gramme of* H_2SO_4 *per* 100 *cubic centims.*

Gold Electrodes.

R in this case $=\frac{15\cdot 0\times 22\cdot 1}{1\cdot 1}=301$ ohms.

Current, in microwebers.	Microwebers per square centimetre of electrode surface.	E, in volts.	CR.	e=E−CR.
7·9	0·46	2·211	·002	2·209
27·8	1·63	2·401	·008	2·393
68·1	4·01	2·483	·020	2·463
130·0	7·65	2·562	·039	2·523
288·0	16·9	2·659	·086	2·573
496·0	29·2	2·775	·149	2·626

Carbon Electrodes.

R now $=\frac{18\cdot 0\times 22\cdot 1}{1\cdot 5}=265$ ohms.

Current, in microwebers.	Microwebers per square centimetre of electrode surface.	E, in volts.	CR.	e=E−CR.
5·6	1·4	1·500	·001	1·499
13·7	3·5	1·614	·004	1·610
25·2	6·4	1·701	·007	1·694
55·4	14·2	1·763	·015	1·748
85·1	21·8	1·823	·023	1·800
248·0	63·5	1·938	·067	1·871

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Fig. 5 (Pl. III.) represents the four curves thus obtained,—those marked 1 and 3 being the curves obtained with acid of 27·0 grammes per 100 cubic centims., and with carbon and gold electrodes respectively; and those marked 4 and 6 the curves obtained with acid of 1·0 gramme per 100 cubic centims., with carbon and gold electrodes respectively. The other two curves traced, marked 2 and 5, are put in for comparison, being respectively the mean curves obtained as above described with acid of 27·0 and 1·0 grammes per 100 cubic centims. with platinum electrodes. In each case the weaker acid gives the higher counter E.M.F. for a given rate of flow of electricity per square centimetre of electrode surface; but the effect produced by the variation in the strength of the acid is small compared with that produced by substituting platinum for carbon, or gold for platinum *.

It should be noticed, however, that it is highly probable that the counter E.M.F. observed with the carbon electrodes is somewhat diminished, at any rate for the smaller currents, by the circumstance that electrolytic "nascent" oxygen is capable of attacking carbon, forming CO and CO_2 (Beetz, Wied. *Annalen*, 1878 [2] v. p. 1; also Phil. Mag. 1879 [5] vii. p. 1); the effect of this would of course be to diminish e by a quantity equivalent to the heat evolved in the oxidation of the carbon (reckoned per gramme-equivalent of water decomposed). With currents of the minuteness of only a few microwebers it was not found practicable to prove directly the formation either of CO or CO_2; but the circumstance that the observed values for e fell slightly below 1·50 volt with the voltameter containing acid of 27 grammes per 100 cubic centims., and with currents of from 5 to 10 microwebers, or slightly above the current equivalent to the "diffusion discharge" found with the same voltameter substituting platinum electrodes for carbon ones (viz. close to 4·0 microwebers), together with the circumstance that with platinum and gold electrodes the value of e was never below 1·5 volt when the current exceeded that equivalent to the "diffusion discharge," would seem to indicate that the values of e for these currents found for the carbon electrodes were actually slightly below the values that would have been found had no oxidation of the electrode taken place.

* The two carbon curves lie a little below their true positions, owing to the effective area of the carbons being a little greater than their superficial measurement, on account of the unavoidable absence of perfect smoothness and slight pitting of the surface. The true area, however, exceeds that measured by only a small amount, the carbon surface having been filed and ground so that the surface was as nearly smooth as it could be made.

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Experiments with Oxidizable Electrodes.

91. Suppose that dilute sulphuric acid is electrolysed with copper electrodes so that a gramme-equivalent of copper sulphate is formed at the + electrode for one of hydrogen evolved at the − electrode. In accordance with the general theorem (§ 61),

$$e=\mathrm{E}_1+[\Sigma\{(1-n)\mathrm{H}\}-\Sigma(nh)-\Sigma(\mathbf{H})]\chi\mathrm{J},$$

the value of e will be

$$e=\mathrm{E}_1+\{(1-n_1)\mathrm{H}_1-n_1h_1-\mathrm{H}_3\}\chi\mathrm{J},$$

where H_3 is the heat evolved during the production of a gramme-equivalent of copper sulphate from copper, ordinary free oxygen, and dilute sulphuric acid; for in this case $\Sigma(\mathbf{H})$ is equal to $\mathrm{H}_3+(1-n_2)\mathrm{H}_2-n_2h_2$ (the n_2, h_2, and H_2 referring to oxygen, and n_1, h_1, and H_1 to hydrogen), the diffusion effect of the copper sulphate solution accumulating round the + electrode being left out of sight.

Writing E_3 for $\mathrm{H}_3\chi\mathrm{J}$,

$$e=\mathrm{E}_1-\mathrm{E}_3+\{(1-n_1)\mathrm{H}_1-n_1h_1\}\chi\mathrm{J}.$$

Of course, if some other metal be used instead of copper, *e. g.* zinc, the same formula will apply, only the value of E_3 will be different.

If, instead of dilute sulphuric acid, a metallic salt, *e. g.* zinc or copper sulphate, be electrolysed, the same formula will hold, E_1 now representing the E.M.F. corresponding to the work done in decomposing the salt electrolysed into oxygen, metal, and dilute sulphuric acid, and E_3, as before, being the E.M.F. representing the work gained in synthesizing the salt produced by the solution of the + electrode from metal, oxygen, and dilute sulphuric acid; since $h_1=0$ when a solid metal is produced,

$$e=\mathrm{E}_1-\mathrm{E}_3+(1-n_1)\mathrm{H}_1\chi\mathrm{J}.$$

Taking into account the diffusive effect due to the strengthening of the metallic salt-solution round the + electrode and its weakening round the − electrode [which, according to the results of Moser (Wied. *Annalen*, iii. p. 216), H. F. Weber (Phil. Mag. [5] viii. pp. 487 and 523), and Helmholtz (Wied. *Annalen*, iii. p. 201), may amount to at least some few hundredths of a volt under certain circumstances], this formula becomes

$$e=\mathrm{E}_1-\mathrm{E}_3+\{(1-n_1)\mathrm{H}_1+h'\}\chi\mathrm{J},$$

where h' is the heat corresponding to the diffusive effect of the concentrated and weak solutions per gramme-equivalent of electrolyte decomposed; h' is of + sign, since the results of

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the above experimenters show that the effect of the different densities of the solutions round the two electrodes is to cause a current to flow through the fluid from the weaker to the stronger solution, *i. e.* from the − to the + electrode *, or in the opposite direction to the current producing electrolysis.

If, then, dilute sulphuric acid, for instance, be decomposed by means of oxidizable electrodes, say of copper, the value of e will, under any given condition, be given by the formula

$$e=\{h'+h''+(1-n_1)H_1-n_1h_1\}\chi J,$$

where $h''\chi J=E_1-E_3$.

The same formula will also apply when a metallic salt is decomposed, $h''\chi J$ still representing E_1-E_3, whatever values these two quantities may happen to possess. If the metallic salt decomposed contain the same metal as that dissolved from the + electrode, E_1 will approximately $=E_3$, and h'' will nearly $=0$. Since, however, the physical condition of the deposited and dissolved metal will rarely, if ever, be identical, E_1 will not exactly $=E_3$; h'' will represent the heat that would be evolved in the transformation of a gramme-equivalent of deposited metal into the physical condition of the dissolved electrode.

Since $h_1=0$ when a solid is deposited, the formula simplifies to this,

$$e=\{h'+h''+(1-n_1)H_1\}\chi J.$$

So-called non-polarizable electrodes are, strictly speaking, misnomers; they are only cases where the value of e is very small, owing to the particular conditions of the experiment rendering $(1-n_1)H_1+h'+h''$ but little greater than 0.

If the value of n_1 decreases as the current increases, when it refers to a metal deposited, as appears to be the case when it refers to hydrogen, from the above described experiments it results that the value of e will continually increase with the current, if the conditions of the experiment are such that h' and h'' remain constant (or if they are negligible).

92. *Decomposition of Acidulated Water with Copper Electrodes.*—In order to examine these points experimentally, known currents were passed through a U-voltameter filled with well-boiled, just cooled, dilute sulphuric acid containing 11·5 grammes of H_2SO_4 per 100 cubic centims., and fitted with copper electrodes each exposing a surface of 8·5 square centimetres. After allowing a current of some 50 to 100

* Presumably this is only the case when heat is *developed* by the mixture of the strong and weak solutions; were heat *absorbed* during the intermixture, probably the current would be reversed in direction; for in that case the expenditure of work in order to effect electrolysis would be less instead of greater when the strong and weak solutions accumulated round the electrodes than it would be did diffusion and intermixture take place instantaneously.

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microwebers to pass for two or three days the values of e became approximately steady; the following numbers were obtained, the electrodes being 13 centimetres apart and the bore of the U-tube between them 1·09 square centimetre, so that the value of $R=\frac{13\times 2\cdot 5}{1\cdot 09}=30$ ohms, 2·5 ohms being (as calculated in § 89, footnote) the specific resistance of sulphuric acid of 11·4 grammes per 100 cubic centims. The values of E are all probably a little underestimated, because it was found impossible to allow a current of 25 to 50 microwebers to pass for a fortnight, so as completely to saturate the acid round the — electrode and the electrode itself with hydrogen, and get rid of the last traces of dissolved oxygen, as was done with platinum, carbon, or gold electrodes; for before a fortnight had elapsed, the copper dissolved from the + electrode and converted into copper sulphate had diffused to the other electrode and became partly re-deposited there (at least this phenomenon was observable in less than a fortnight when a polished platinum plate was used instead of a copper one for the — electrode). Probably this source of error affects all the observations about equally, and is but slight, since sensibly the same values of E were observed for a given current whether the observations were made on the 4th or on the 9th day.

Current, in microwebers.	Microweber per square centimetre of electrode surface.	E, in volts.	CR.	$e=E-CR.$
9·7	1·14	·523	...	·523
20·1	2·34	·548	·001	·547
49·8	4·23	·586	·001	·585
77·1	9·06	·600	·002	·598
108·7	12·4	·616	·003	·613
174·7	20·5	·669	·005	·664
226·0	26·6	·685	·007	·678
378·0	44·5	·720	·011	·709

These numbers clearly show that the values of $h'+(1-n_1)H_1-n_1h_1$ for currents of not less than 1·14 microweber per square centimetre of electrode surface are positive, and increase as the current increases; for h'' cannot exceed 0·4 volt, and the sum of h'' and $h'+(1-n_1)H_1-n_1h_1$ is uniformly greater than this. [That h'' is not $>0\cdot 4$ volt results from the valuations of E_3 cited in Part I. § 21 and 22, deduced from the experiments of Favre, Julius Thomsen, and Raoult, which varied between 1·162 and 1·321, being uniformly above 1·1 volt, so that, as $E_1=1\cdot 50$ volt (Part II.), $h''=E_1-E_3$ cannot exceed 0·4 volt.] This increase is mainly due to the decrease in value of n_1 as the current increases; for

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on altering the current-strength to some value previously experimented with, the value of e always immediately altered to something very close to the value it previously was found to possess for the altered current, which could not have been the case had the increase in value of e found for a stronger current been simply due to the increase in the value of h' owing to the accumulation of copper sulphate solution round the + electrode.

Very little difference in the value of E was noticed when gold was substituted for copper as the negative electrode (the dimensions and the positive electrode remaining unchanged), the curve deduced by plotting the values of $E-CR$ as ordinates and the rates of flow per square centimetre as abscissæ being nearly, though not quite, the same whether the — plate were copper or gold; what little difference did exist was of such a nature that the gold — electrode curve lay slightly above the copper one. On substituting platinum for copper, the curve was very distinctly depressed; *i. e.* the values of $E-CR$ for a given rate of flow and a constant copper positive electrode were smaller with a platinum electrode than with a copper one, and smaller with a copper one than with a gold one. Whether the values thus registered by the aid of the quadrant-electrometer are strictly comparable, or whether they are complicated by a difference of contact action taking place between the electrode and the brass quadrant of the electrometer (by means of the connecting-wire), is, however, open to discussion *. Admitting that the contact action does not materially affect the values obtained, the numbers found with the gold, copper, and platinum — electrodes would indicate that the value of n_1 is greater with platinum than with copper and greater with copper than with gold; *i. e.* the "condensing" actions of the three metals for "nascent" hydrogen lie in the order, platinum greatest, then copper, and lastly gold. As regards platinum and gold, this is precisely the conclusion to be drawn from the experiments described in § 90.

Experiments bearing further on the phenomena observed

* It is manifest that, whatever may be the nature of the contact actions between two dissimilar metals &c., and however this may affect the determination of the true difference of potential between two dissimilar plates connected respectively with the two quadrants of an electrometer, such actions have no influence at all on the result when the two metals are the same; so that whilst the mercury in a mercury-cup and two thick copper wires dipping into it may differ in potential, yet the two wires will be at the same potential, *i. e.* just away from the mercurial surface; and the difference of potential between the two quadrants of an electrometer is the same as that between the two plates of a voltameter &c. respectively connected with them when these plates are of the same material, *e. g.* platinum, although the intermediate connecting-wires and plates themselves may differ in their absolute potentials from the quadrant-potentials.

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when the electrodes are not of the same material, will be considered in a future paper.

93. *Decomposition of Copper Sulphate with Copper Electrodes, and of Zinc Sulphate with Zinc Electrodes.*—The voltameters used consisted simply of U-tubes filled with the hot, just boiled, solution of zinc or copper sulphate employed, and then closed by india-rubber stoppers through which passed glass tubes with platinum wires through the drawn-out ends, so as to make mercury-cups, and keep the solution from access of air; to the platinum wires were soldered the electrodes, consisting of well-brightened strips of metal of known size, the solderings and platinum wire being covered over with gutta percha. By introducing a known resistance into the circuit, composed of voltameter and electromotor (one or more De la Rue, or other analogous cells), the current could be regulated and measured, and hence the value of $e=E-CR$ determined. In this way the following numbers were obtained. With the copper-sulphate voltameter (which contained a nearly saturated solution of the salt, containing 7·84 grammes metallic copper per 100 cubic centims., the formula $CuSO_4$ $36H_2O$ requiring 7·80) the value of R was found to be close to 110 ohms at the average temperature of the observations—this number being calculated from the dimensions of the column of fluid between the electrodes, and the specific resistance of the fluid determined by a new method which will be described in detail in a future paper. Similarly, the value of R for the zinc-sulphate voltameter was calculated to be close to 250 ohms.

The following numbers were obtained with copper-sulphate and copper electrodes:—

Surface of each electrode = 8·65 square centimetres.

(I.) *Values obtained when each electrode was new*, i. e. *polished compact rolled copper plate.*

Current, in microwebers.	Microwebers per square centimetre of electrode surface.	E, in volts.	CR.	$e=E-CR$.
7·1	0·8	·032	·001	·031
14·6	1·7	·033	·002	·031
23·8	2·7	·036	·003	·033
30·8	3·6	·036	·003	·033
35·7	4·1	·039	·004	·035
73·0	8·4	·047	·008	·039
148·0	17·1	·060	·016	·044
185·8	21·5	·062	·020	·042
247·0	28·6	·073	·027	·046
371·0	42·9	·088	·041	·047
728·0	84·2	·131	·080	·051

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(II.) *Values obtained with electro-deposited copper on each electrode.*

(*a*) When the copper had been deposited some 48 hours, the electrodes being in the voltameter out of contact of air:—

15·2	1·7	·018	·002	·016
30·2	3·5	·025	·003	·022
91·5	10·6	·036	·010	·026
212·0	24·7	·050	·023	·027
403·0	46·6	·079	·044	·035
749·0	86·6	·137	·082	·055

(*b*) When the copper had not been deposited so long, from 15 to 20 hours after deposition :—

15·2	1·7	·015	·002	·013
22·8	2·6	·017	·003	·014
30·2	3·5	·020	·003	·017
45·0	5·2	·024	·005	·019
89·1	10·3	·033	·010	·023
253·0	29·2	·052	·028	·024
420·0	48·6	·078	·046	·032
774·0	89·6	·130	·085	·045
1328·0	153·5	·205	·146	·059

(*c*) When the copper had only been deposited a few minutes before each observation was made :—

16·6	1·9	·011	·002	·009
28·4	3·3	·014	·003	·011
42·3	4·9	·018	·005	·013
134·5	15·5	·031	·015	·016
240·0	27·7	·048	·026	·022
383·0	44·3	·070	·042	·026
740·0	85·5	·116	·081	·035
1311·0	151·6	·190	·144	·046

By interpolation, the following values for *e* are calculated from the above figures :—

Current, in microwebers.	I.	II*a*.	II*b*.	II*c*.
10	·031	—	—	—
20	·032	·017	·014	·010
50	·037	·023	·020	·013
100	·041	·026	·023	·015
200	·043	·027	·024	·020
500	·049	·043	·036	·029
1000	—	—	·050	·040

From which it is evident that the more freshly deposited the electro-copper was, the less was the value of *e*,—leading to

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the conclusion that either the freshly deposited copper had a higher power of "condensing" copper on its surface than rolled copper, thereby increasing the value of n_1 in the formula

$$e = \{h' + (1-n_1)H_1 + h''\}\chi J,$$

and that this power became lessened on standing, or else that, on standing, the surface of the copper altered in such a way as to cause the value of h'' to increase, or, possibly, that both changes took place together.

Precisely analogous results were obtained in other experiments with different voltameters: with considerably stronger currents up to some 5000 microwebers per square centimetre of electrode surface, values of 0·2 volt and upwards have been obtained for e by a method now being elaborated in conjunction with Mr. E. H. Rennie. Admitting that in these instances the values of h' and h'' are negligible compared with $(1-n_1)H_1$ (which probably is not quite the case), it results that the value of $H_1\chi J$ must exceed 0·2 volt, *i. e.* that H_1, the heat of transformation of "nascent" into ordinary copper, is greater than $\frac{0{\cdot}2 \times 10^8}{4410} = 4500$ gramme-degrees per gramme-equivalent.

94. In precisely the same way the following values were obtained on electrolysing a solution of zinc sulphate with bright zinc electrodes, the area of each of which was 8·6 square centims., the value of R being close to 250 ohms.

Current, in microwebers.	Microwebers per square centim. of electrode surface.	E, in volts.	CR.	e=E−CR.
14·3	1·7	·023	·004	·019
28·2	3·3	·031	·007	·024
39·4	4·6	·039	·010	·029
69·8	8·1	·050	·017	·033
87·9	10·2	·068	·022	·046
136·4	15·9	·085	·034	·051
167·1	19·4	·094	·042	·052
310·0	36·0	·131	·078	·053
647·0	75·2	·232	·162	·070
1147·0	132·5	·365	·287	·078

The curves traced out with zinc- and copper-sulphate solutions and zinc and copper poles respectively exhibit resemblance in general characters to those obtained with acidulated water and non-oxidizable electrodes: they indicate that e increases with the current but not so rapidly, so that the curves are concave downward.

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95. Under certain conditions, the value of h'' in the formulæ arrived at in § 91, representing the counter E.M.F. set up during electrolysis, may become negative, but numerically greater than $h'+(1-n_1)H_1-n_1h_1$. In such a case negative work must be spent (or work will be gained) during the decomposition; the cell is then termed an electromotor, and constitutes an ordinary galvanic element. When this is the case, $E_2 > E_1$,—as, for instance, when the + electrode is zinc and dilute sulphuric acid is electrolysed, or when copper sulphate is electrolysed as a partitioned cell, the one half containing zinc sulphate with a plate of zinc therein constituting the + electrode (Daniell's cell). The previous experiments show that the value of the expressions

$$h'+(1-n_1)H_1-n_1h_1 \quad \text{and} \quad h'+(1-n_1)H_1$$

(according as a gas or a solid is evolved at the negative electrode) are positive even with very small currents, and increase as the current increases. It hence results that the numerical value of the negative counter E.M.F. set up in a simple voltaic circuit of zinc, another metal, and dilute sulphuric acid, or in a Daniell's cell (*i. e.* the actual E.M.F. of the cell*), *must decrease as the current produced by it increases.*

In the case of a simple voltaic circuit, this effect is wholly distinct from one often observed in a newly set-up cell charged with dilute acid containing dissolved air. When first set to work the action of the cell is greatly intensified by the oxidation of the evolved hydrogen by the dissolved oxygen (much as is the case in a Leclanché element, the oxygen coming from

* The E.M.F. of an electromotor is defined for the present purpose as *the difference of potential set up between two poles of the same material* (*e. g.* the two brass quadrants of an electrometer, or two copper conducting-wires, the one connected with one plate of the electromotor, the other with the other) *corrected for the effect of the current passing (if appreciable) due to the external resistance.* Whatever may be the influence of contact actions upon this corrected potential-difference in any given case, this influence must be constant for a given apparatus, and independent of the current generated, so that any change in the potential-difference set up is not due in any way to contact-action between the metallic plates and conducting-wires &c. The correction is of this form :—Let E_1 be the actual difference of potential and E_2 the E.M.F. of the electromotor, let R_1 be the external resistance and R_2 the resistance of the electromotor, and let C be the current passing (which $=0$ if R_1 is indefinitely great); then $E_2=\frac{R_1+R_2}{R_1}$, $E_1=E_1+CR_2$. For the total work done by the current in a time t is $C^2(R_1+R_2)t$, which $=E_2Ct$, whilst the work done outside the electromotor is $C^2R_1t=E_1Ct$; whence

$$E_2=E_1\times\frac{R_1+R_2}{R_1}=E_1+R_2\times\frac{E_1}{R_1}=E_1+CR_2.$$

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the manganese dioxide); but by-and-by this ceases and the E.M.F. goes down. This effect is often said, vaguely and not strictly correctly, to be due to the plates becoming "polarized" by hydrogen, the which phrase really applies to the wholly different phenomenon now under consideration.

That the E.M.F. of a Daniell cell is somewhat less when generating a current than when no current passes has already been shown to be the case by Waltenhofen (*Sitzb. Akad. Wien*, xlix. 2, p. 229) and by Beetz, using a modification of the method of compensation (Pogg. *Ann.* cxlii. p. 581, 1871). Apparently, however, physicists generally are under the impression that the diminution in E.M.F., or "polarization" of the cell, thus produced is practically inappreciable: thus, for instance, within the last few months a determination of the value of v has been made in Sir W. Thomson's laboratory by R. Shida, by measuring the E.M.F. of a gravity Daniell, first, in electrostatic units (no current being generated), and then in electromagnetic units (a moderately powerful current being set up); the which method evidently can only be applied if the difference in the E.M.F. according as a current is generated or not is negligible (Brit. Assoc. Reports, 1880, p. 497). The same kind of remark applies to several other investigations published during the last few years.

96. *Decomposition of Dilute Sulphuric Acid with a Zinc Positive Electrode.*—On trying experiments with a zinc (amalgamated) positive electrode and a platinum electrode in the same way as those described in § 92, it was found that the numerical value of e was uniformly negative and decreased as the current increased. So long as the current remained of a constant value the nature and dimensions of the external battery were immaterial; so that *the same value for* e *was obtained whether an external battery was actually applied, or whether the zinc-platinum couple was the sole electromotor in circuit*, provided that, by suitably adjusting the total resistance in circuit, the current was made the same. Thus the following average numbers were obtained with a cell containing dilute sulphuric acid of 10 grammes H_2SO_4 per 100 cubic centims., the plates each exposing 8·6 square centims. of surface, and being contained in a U-voltameter like those above described, of 1·1 square centim. bore, the centres of the plates being 13 centims. apart; so that, as the specific resistance of sulphuric acid of the strength used is close to 2·9 ohms at 18° (from Kohlrausch's numbers, *loc. cit. suprà*), the value of R was close to $\frac{13 \times 2{\cdot}9}{1{\cdot}1} = 34$ ohms:—

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Current, in microwebers.	Microwebers per square centimetre.	E, in volts.	CR.	$-e=-(E-CR)$.
6·7	0·78	−·604	...	·604
9·8	1·14	−·589	...	·589
14·4	1·67	−·571	...	·571
23·2	2·7	−·547	·001	·548
41·0	4·8	−·519	·001	·520
58·5	6·8	−·493	·002	·495
96·0	11·2	−·442	·003	·445
135·6	15·8	−·395	·005	·400

Evidently the value of $-(E-CR)$—that is, the direct E.M.F. of the combination—decreases as the current increases, but not at the same rate, the diminution in the E.M.F. being less rapid than the increase in rate of flow.

97. *Daniell's Cell.*—In order to verify the deductions above made from the preceding experiments as to the variation of the E.M.F. of a Daniell's cell with the current produced, and so to amplify the observations of Waltenhofen and Beetz, the following experiments were made with two such cells arranged on the "gravity" principle—*i. e.* no porous cell being employed, but the copper plate being lowest and immersed in a saturated solution of copper sulphate, the zinc plate being uppermost and immersed in a solution of zinc sulphate of lower specific gravity than the copper sulphate solution. In the first cell the active surface of each plate was about 15 square centims., and the resistance of the column of fluid between them was calculated, from the dimensions of the column and Kohlrausch's values for the specific resistances of copper and zinc sulphate solutions, to be about 4 ohms. In the second cell much smaller plates were used, exposing about 1 square centim. of active surface, the resistance of the column of fluid being calculated to be about 40 ohms.

First Cell.

Current, in microwebers.	Microwebers per square centimetre.	E, in volts.	CR.	$-e=-(E-CR)$.
0	0	−1·090	...	1·090
10·9	0·7	−1·090	...	1·090
36·3	2·4	−1·090	...	1·090
107·7	7·2	−1·077	...	1·077
267·2	17·8	−1·069	·001	1·070
530·5	35·4	−1·061	·002	1·063
1317·5	87·8	−1·054	·005	1·059

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Second Cell.

Current, in microwebers.	Microwebers per square centimetre.	E, in volts.	CR.	$-e=-(E-CR)$.
0	0	−1·105	...	1·105
11·2	11	−1·105	...	1·105
36·8	37	−1·094	·001	1·095
109·1	109	−1·082	·004	1·086
265·5	266	−1·053	·010	1·063
515·5	516	−1·020	·021	1·041
1140·0	1140	−0·982	·046	1·028

In these experiments the correction CR is only approximate, it being impossible to calculate the precise value of R with any great degree of accuracy; but evidently, except in the last one or two determinations, even a large error in the value of R would make no appreciable difference in the end result, viz. *that the E.M.F. of a Daniell cell regularly decreases as the current produced increases, but not at so rapid a rate.*

During the time that these observations were being made, the changes in the E.M.F. of the cell due to "running down" and variation of the density of the solutions surrounding the two plates respectively, were too small to be capable of measurement. After making an observation with a larger current and reducing the current to a smaller value or to *nil*, sensibly the same value was always obtained whether at the beginning or end of the series of measurements, which lasted some two or three hours in each case. If, however, observations were made one day and then repeated a day or two after, concordance was rarely noticeable between the results: the later experiments always showed a more rapid rate of fall in the numerical value of $-e$ with the current-increase than the earlier ones, and also generally indicated a perceptibly lower value when the current was *nil*. Apparently this result was due to the deposition of copper on the surface of the zinc plate, owing to diffusion.

Experiments are now in progress in which the true value of R for any given cell is determined by a new method: so far these have given results entirely confirming those above described. The highest values hitherto obtained for the E.M.F. of a freshly arranged Daniell's cell generating an indefinitely minute current only, and freed from complicating circumstances as far as possible (especially presence of dissolved air, which tends slightly to lower the value), are between 1·105 and 1·110 volt (Clark's cell being 1·457 volt) at the temperature of 15° or thereabouts. The discussion of these experi-

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ments and of the valuations of the E.M.F. of a Daniell cell made by previous observers, and of the bearing on the discrepancies between these valuations of the point just substantiated (viz. the dependence of the E.M.F. on the current generated) will be resumed in a future paper. It may be noticed, however, that even with a not very enormous rate of current-flow per unit area, the diminution in E.M.F. amounts to several per cent. of the highest value, a quantity quite sufficient to vitiate observations in which the E.M.F. is assumed to remain constant.

98. Similarly the E.M.F. of a Grove's cell is not wholly independent of the current generated, although the effect is somewhat less marked than with a Daniell cell. The end result of the chemical changes taking place in this cell are expressed by one or other of the equations

$$Zn + H_2SO_4 + 2HNO_3 = ZnSO_4 + 2H_2O + 2NO_2,$$
$$2Zn + 2H_2SO_4 + 2HNO_3 = 2ZnSO_4 + 3H_2O + N_2O_3,$$
$$3Zn + 3H_2SO_4 + 2HNO_3 = 3ZnSO_4 + 4H_2O + 2NO;$$

so that, in addition to the production of gases (more or less completely absorbed by the nitric acid), dilution of the nitric acid takes places in the proximity of the platinum plate, thereby causing h' (the correction for the effects of differences in the density of the fluids surrounding the electrodes) to have an appreciable value. It is therefore to be expected that the E.M.F. of a Grove's cell will, like that of a Daniell's or an ordinary voltaic circle, decrease with the current generated. That this is actually the case is indicated by the following figures obtained with a "gravity" Grove's cell, the plates of which exposed an actual surface of 8 square centims. each, the calculated resistance of the column of fluid (partly concentrated nitric acid, partly 5-per-cent. sulphuric acid solution) between the plates being about 2 ohms, and the zinc plate being well amalgamated.

Current, in microwebers.	Microwebers per square centimetre.	E, in volts.	CR.	$-e = -(E-CR)$.
0	0	−1·872	...	1·872
9·4	1·2	−1·872	...	1·872
18·7	2·3	−1·872	...	1·872
36·4	4·6	−1·872	...	1·872
93·6	11·7	−1·872	...	1·872
187·2	23·4	−1·872	...	1·872
466·2	58·3	−1·867	·001	1·768
930·5	116·3	−1·861	·002	1·863
1850·0	231·3	−1·850	·004	1·854
3678·0	459·8	−1·839	·007	1·846
8955·0	1119·4	−1·791	·018	1·809
17480·0	2185·0	−1·747	·035	1·782

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99. It hence results that all methods for determining the E.M.F. of voltaic combinations, or for determining the internal resistance of cells, which depend on the production of currents of different magnitudes by means of the voltaic combination employed, may be erroneous to a considerable extent when it is assumed (as is ordinarily done) that the E.M.F. of the combination is independent of the current generated, and when the voltaic combination is such that this assumption is untrue, which is the case apparently in all the more common forms of cells, if indeed it is not universally the case. Thus, for instance, in Ohm's method for determining the E.M.F. of a cell, the current C_1 produced by the cell when connected with a galvanometer, the total resistance in circuit being R_1, is determined; an additional resistance R_2 is then introduced, and the new current C_2 again determined. If the galvanometer be a sine-galvanometer, and α and β are the deflections in the two cases respectively,

$$C_1 = K \sin \alpha \text{ and } C_2 = K \sin \beta,$$

where K is a constant for the instrument. If the E.M.F. of the cell be supposed to be the same (E) for both currents, then

$$C_1 = K \sin \alpha = \frac{E}{R_1},$$

and

$$C_2 = K \sin \beta = \frac{E}{R_1 + R_2};$$

whence

$$E = K \,.\, R_2 \frac{\sin \alpha \,.\, \sin \beta}{\sin \alpha - \sin \beta}$$

$$= C_1 R_2 \frac{\sin \beta}{\sin \alpha - \sin \beta}$$

$$= C_2 R_2 \frac{\sin \alpha}{\sin \alpha - \sin \beta};$$

that is, E is known in terms of the currents, the deflections, and the added resistance R_2.

But if E be not the same for the two currents, being greater for the smaller by an amount δE, the equations become

$$C_1 = K \sin \alpha = \frac{E}{R_1},$$

and

$$C_2 = K \sin \beta = \frac{E + \delta E}{R_1 + R_2} = \frac{(1+\delta)E}{R_1 + R_2};$$

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whence

$$(1+\delta)E = K \cdot \sin\alpha \cdot R_2 \frac{\sin\beta}{\sin\alpha - \frac{\sin\beta}{1+\delta}}.$$

Evidently, if δ be assumed $=0$, the value of this expression is greater than the value when δ is not $=0$; that is, the assumption that the E.M.F. of the cell is constant throughout leads to the calculation of a value for its E.M.F. greater than the true value for the smaller current, and *à fortiori* greater than the true value for the larger current*.

In just the same way an incorrect value is obtained for the internal resistance of a cell by Ohm's method, which depends on the same principles. If R_1 be the resistance of the cell and R that of the rest of the circuit, a current is generated

$$C_1 = K \sin\alpha = \frac{E}{R+R_1},$$

and by throwing in an extra resistance R_2 the current is reduced to

$$C_2 = K \sin\beta = \frac{E(1+\delta)}{R+R_1+R}.$$

If it be assumed that $\delta=0$, then

$$R_1 = R_2 \frac{\sin\beta}{\sin\alpha - \sin\beta} - R;$$

but if δ is not equal to 0, then

$$R_1 = R_2 \frac{\sin\beta}{(1+\delta)\sin\alpha - \sin\beta} - R,$$

which is manifestly less than the former value; so that the assumption that the E.M.F. of the cell is constant throughout leads to the overvaluation of its internal resistance.

100. The results above described have an important bearing on a considerable amount of work performed by Favre (*Comptes Rendus*, 1866 and following years). By passing a current through an electrolytic cell placed in a calorimeter, an external resistance being in circuit so large

* In H. F. Weber's experiments on the value of J (§ 55), a trifling source of error exists in those observations made with a voltaic battery, the mean E.M.F. of which during the experiment was measured as a necessary part of the data; for the measurement gives the E.M.F. corresponding to a current C_1, whilst the heat evolved was that caused by a different current C_2. As, however, Weber states that C_1 was always approximately $=C_2$, this source of error is not great and is probably negligible, since the B.A. resistance values got by this method are sensibly the same as those obtained by the two other independent methods employed.

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that the resistance of the cell was wholly negligible (?) in comparison with it, and consequently the heat developed in accordance with Joule's law was practically *nil*, Favre obtained values in terms of heat corresponding to the difference between the work done in decomposing the electrolyte into the primary and the final products of decomposition. *E. g.* in one set of experiments with water, the actual mean heat-development was close to 14,000 gramme-degrees per gramme-equivalent of water decomposed; adding to this 34,100 gramme-degrees, the heat corresponding to the formation of gaseous oxygen and hydrogen (Part I. § 31), it results that in this experiment the total work done during the electrolysis was equivalent to 48,100 gramme-degrees, or 2·12 volts; *i. e.* the mean value of e in these experiments was 2·12 volts. By comparing together various values obtained in this way with different electrolytes, Favre was led to the conclusion that of this 14,000 gramme-degrees (which he regarded as a constant heat-absorption during water-electrolysis over and above the heat taken up in the formation of gaseous oxygen and hydrogen =34,100), about 9500 were due to the transformation of allotropic oxygen formed by the first action of the current into ordinary oxygen gas, and the remainder to the transformation of allotropic hydrogen into ordinary hydrogen gas. The above-described results, however, indicate that these particular numerical values have only reference to the particular apparatus and current-strengths used by Favre: had the size of the electrodes been varied, or their nature, or the current-strength, quite different numbers would have been obtained in each case, not only in these particular experiments cited, but also in a number of other analogous ones with other substances; for each change would have brought about a corresponding change in the value of e, and hence in the heat-evolution. Apart from this, however, as Favre's values were obtained with the mercury-calorimeter, a considerable probable error attends the determinations; so that finally no reliance at all can be placed on the conclusions arrived at by Favre in this series of experiments, as regards numerical values.

It is noticeable in this connexion, that experiments by Raoult (*Annales de Chimie et de Phys.* ii. p. 317, and iv. p. 392, 1864 and 1865) show that the value of the counter electromotive force set up during electrolysis varies with the current *cæteris paribus*. Raoult observed the "subsequent polarization" of an electrolytic cell very quickly after the rupture of the current, determining it in terms of a Daniell's cell, this being itself valued in terms of the heat producible by

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its current whilst a gramme-equivalent of copper was precipitated. The difference between the heat actually evolved in the cell (measured by a calorimeter) and that equivalent to the polarization, was termed by Raoult "Local Heat," and found by him to be larger with stronger currents. The results above described with a Daniell cell, however, show that the valuation in terms of heat of a Daniell cell is not a constant quantity; for the heat capable of being evolved by the current outside the battery during the precipitation of a gramme-equivalent of copper, being proportional to the E.M.F. of the cell, is variable with the time during which this precipitation takes place, *i. e.* with the strength of the current; whilst the "subsequent polarization," measured by Raoult, being always more or less inferior to the counter E.M.F. set up during the passage of the current, is not the true measure of the electrolytic work done by the current whilst it was passing through the cell.

Notwithstanding, however, that the numerical results of Favre and Raoult are open to exception, these observers were amongst the first, if not actually the first, to attempt to carry out experimentally some of the lines of research opened out by Sir W. Thomson (1851 paper), although the theoretical aspects of the question, as discussed by them, do not entirely coincide with Thomson's mode of reasoning.

Summary of Results.

101. The following summary briefly indicates the salient results arrived at in Parts III. and IV. of these researches.

(1) The passage of a given quantity of electricity through an electrolyte causes the decomposition of one and the same amount of substance, irrespective of the time taken in its passage; in other words, conduction without electrolysis does not take place, and Faraday's law is true for excessively minute currents as well as for those of considerable magnitude.

(2) With very feeble currents, however, and with certain electrolytes, *e. g.* water, the quantity of products of decomposition actually collected after a given time, does not absolutely correspond to the quantity of electricity that has passed, even after various sources of suppression have been eliminated, such as occlusion in or condensation on the electrodes, solution in the fluid, or suppression by the chemical action of dissolved gases. The cause of this is the "diffusion discharge" produced (in the case of water) by the diffusion towards the − electrode of water containing dissolved oxygen, and towards the + electrode of fluid containing dissolved hydrogen (and similarly in other cases), thus causing an unavoidable

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suppression by chemical action. When the amount of suppression due to this cause is determined and added to the observed amount of decomposition, the total corresponds exactly with the quantity of electricity that has passed.

(3) In the electrolysis of acidulated water, until the sources of loss of hydrogen other than "diffusion discharge" are eliminated (viz. solution in the fluid, condensation and absorption by the electrode, and action of dissolved oxygen originally present in the fluid), the counter E.M.F. set up when a given steady current traverses a given voltameter is short of its maximum value for that current; simultaneously a deficiency in the amount of hydrogen collected as compared with that due to the quantity of electricity passing is noticed (even after correction for diffusion discharge); whilst on breaking circuit, the rate of fall of the "polarization" of the electrodes is more rapid than its minimum value for that current. On the other hand, as soon as the counter E.M.F. reaches its maximum, the deficiency in hydrogen disappears (after correction for diffusion discharge), and the rate of fall of the polarization after breaking circuit reaches its minimum. The more nearly completely the sources of loss are eliminated, the more nearly does the counter E.M.F. set up approach its maximum, the less is the deficiency in the hydrogen collected, and the more nearly does the rate in fall of polarization approach its minimum.

(4) A theory has been enunciated, and more or less completely demonstrated, which reconciles the hitherto apparently contradictory numerical values obtained by previous observers for the counter E.M.F. set up during electrolysis, the "subsequent polarization" existing after rupture of the current, and the E.M.F. of gas batteries. In accordance with this theory, the relationships between the current flowing and the counter E.M.F. set up in an electrolytic cell are expressed by the following formula :—

$$e=\mathrm{E}+[\Sigma\{(1-n)\mathrm{H}\}-\Sigma(nh)-\Sigma(\mathbf{H})]\chi\mathrm{J}.$$

(5) In the case of all the electrolytes examined, the value of e is found to increase as the current increases, but at a less rapid rate, so that the curves traced out by plotting currents as abscissæ and counter E.M.F. values as ordinates are concave downwards.

So long as the rate of flow per unit area of electrode surface remains the same, the value of e is constant; *i. e.* if the electrode surface and the current vary in the same way, then e remains the same. With a given current increasing the electrode surface diminishes the value of e.

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The values of e depend on the material of which the electrodes are made. Other things being equal, carbon gives higher values than platinum, and platinum than gold.

In the case of acidulated water, rendering the solution more dilute increases the value of e.

From former observers' work it results that increasing the temperature decreases e.

(6) The above formula, in conjunction with experimental determinations of the variations in the values of e under various conditions, explains coherently and quantitatively the leading facts connected with counter E.M.F., polarization, and the E.M.F. of gas-batteries, and accounts for the possibility, under certain conditions, of decomposing an electrolyte with a less battery E.M.F. than corresponds to the work done in splitting it up into the final products—and also indicates that the E.M.F. of a Daniell cell is not constant, but is less the greater the current it generates, to such an extent that with currents of not very great magnitude a diminution in E.M.F. to the extent of several per cent. is brought about, and that, in consequence, certain of the methods in use for determining the E.M.F. of cells and their internal resistance are, at any rate for certain kinds of cells, incorrect, as they are based on the assumption that the E.M.F. of the cell is constant.

(7) By means of the above formula and the experimental determinations of the causes which make e to vary, &c., information can be deduced as to the amount of energy with which gases are condensed upon the surface of solids, and in particular as to the amount of energy requisite to break up an electrolyte into "nascent" products solely. By conjoining this determination with the determination of the methods for finding the E.M.F. corresponding to the energy requisite to break it up into the final products (described in Parts I. and II.), information can be deduced concerning the affinity of the constituents of the electrolyte, and the variations of that affinity according as the constituents are "nascent" or in their ordinary free state. For instance, the energy given out in the transformation of nascent hydrogen and oxygen into the ordinary free gases exceeds the energy that would be developed by the union of these gases to form liquid water, *i. e.* exceeds the energy equivalent to 34100 gramme-degrees or to 1·50 volt.

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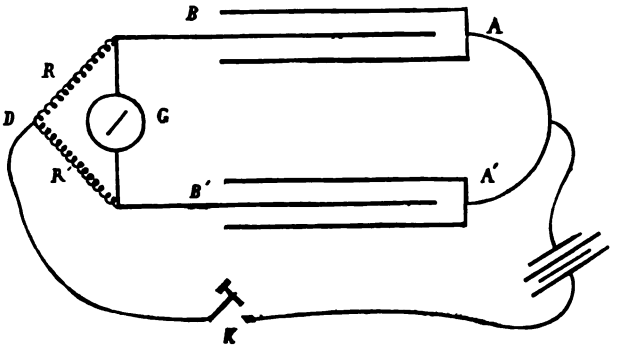
XLIX. *On a Method of Comparing the Electrical Capacities of two Condensers.* By R. T. Glazebrook, *M.A., Fellow of Trinity College, and Demonstrator of Experimental Physics at the Cavendish Laboratory, Cambridge* *.

THE following is a well-known method of comparing the capacities of two condensers :—

Let A, A′ be the outer coatings, B, B′ the inner of two condensers. Connect A A′ together, and to one pole of a battery. Connect B to a resistance R and to one pole of a



galvanometer, B′ to another resistance R′ and to the other pole of the galvanometer. Connect the other ends of R R′ together and to a key K, and let the second screw of the key be in connexion with the other pole of the battery. Let C C′ be the capacities of the condensers. On depressing the key the condensers are charged; and it is easy to show that, if $CR = C'R'$, no current passes through the galvanometer.

If, then, we adjust R until no current is observed on making contact, R′ remaining unaltered, we can find the ratio of C to C′.

I propose to discuss the more general problem of finding the current through the galvanometer when the equation $CR = C'R'$ is not fulfilled, and hence to obtain the conditions of sensibility.

Let V_1 be the potential of A, V_2 of the other pole of the battery, V of B, V′ of B′, at time t. Let G be the galvanometer resistance.

Let ι = current in R;
ι' = „ R′;
ι_1 = current into condenser A;
ι_1' = „ „ B;
x = current through galvanometer;
Q, Q′ = the quantities in the condensers.

* Communicated by the Physical Society, having been read at the Meeting on January 22.

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Let us further suppose that there is a small leakage through the condensers, $\rho\,\rho'$ being their resistances.

Then we have

$$\left.\begin{aligned} \iota &= \frac{V_2-V}{R}, \quad \iota'=\frac{V_2-V'}{R'}, \quad x=\frac{V-V'}{G}, \\ \iota_1 &= \iota-x, \quad \iota_1'=\iota'+x, \\ Q &= C(V-V_1), \quad Q'=C'(V'-V_1), \\ \iota_1 &= \frac{dQ}{dt}+\frac{V-V_1}{\rho}, \\ \iota_1' &= \frac{dQ'}{dt}+\frac{V'-V_1}{\rho'}, \end{aligned}\right\} \quad . \quad . \quad (1)$$

From these we obtain:—

$$Gx+R\iota-R'\iota'=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$CR\frac{d\iota}{dt}+\frac{R+\rho}{\rho}\left\{\iota-\frac{V_2-V_1}{R+\rho}\right\}-x=0; \quad . \quad . \quad . \quad (3)$$

$$C'R'\frac{d\iota'}{dt}+\frac{R'+\rho'}{\rho'}\left(\iota'-\frac{V_2-V_1}{R'+\rho'}\right)+x=0. \quad . \quad . \quad . \quad (4)$$

Assume

$$\begin{aligned} \iota-\frac{V_2-V_1}{R+\rho} &= Ae^{-nt}, \\ \iota'-\frac{V_2-V_1}{R'+\rho'} &= A'e^{-nt}, \\ x &= Be^{-nt}. \end{aligned}$$

On substituting we have

$$A\left\{\frac{R+\rho}{\rho}-CRn\right\}-B=0, \quad . \quad . \quad . \quad . \quad (5)$$

$$A'\left\{\frac{R'+\rho'}{\rho'}-C'R'n\right\}+B=0, \quad . \quad . \quad . \quad (6)$$

$$GB+RA-R'A'=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

whence

$$\left.\begin{aligned} A\left\{G\left(\frac{R+\rho}{\rho}-RCn\right)+R\right\}-R'A'=0, \\ A'\left\{G\left(\frac{R'+\rho'}{\rho'}-R'C'n\right)+R'\right\}-RA=0. \end{aligned}\right\} \quad . \quad (8)$$

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Eliminating A A′ we arrive at the quadratic,

$$n^2-n\left\{\frac{1}{G}\left(\frac{1}{C}+\frac{1}{C'}\right)+\frac{1}{R'C'}\left(\frac{R+\rho}{\rho}\right)+\frac{1}{RC}\left(\frac{R'+\rho'}{\rho'}\right)\right\}$$
$$+\frac{1}{GRR'CC'}\left\{\frac{G(R+\rho)(R'+\rho')}{\rho\rho'}+\frac{R(R'+\rho')}{\rho'}\right.$$
$$\left.+\frac{R'(R+\rho)}{\rho}\right\}=0. \quad \ldots \quad (9)$$

Let n_1 n_2 be the roots, and let

$$\left.\begin{aligned} G\left(\frac{R+\rho}{\rho}-RCn\right)+R&=\lambda,\\ G\left(\frac{R'+\rho'}{\rho'}-R'Cn\right)+R'&=\lambda'.\end{aligned}\right\} \quad (10)$$

Then from (8) we have

$$\left.\begin{aligned} \lambda_1 A_1 + \lambda_2 A_2 &= R'(A_1'+A_2'),\\ \lambda_1' A_1' + \lambda_2' A_2' &= R(A_1+A_2),\end{aligned}\right\} \quad (11)$$

where A_1, λ_1 &c. denote the values of A, λ corresponding to n_1, n_2.

Also initially

$$\iota=\frac{V_2-V_1}{R}, \quad \iota'=\frac{V_2-V_1}{R'}; \quad \ldots \quad (12)$$

therefore, putting $t=0$ and $V_2-V_1=E$ in the equations

$$\iota=\frac{V_2-V_1}{R+\rho}+A_1e^{-n_1t}+A_2e^{-n_2t}$$
$$\iota'=\frac{V_2-V_1}{R'+\rho'}+A_1'e \quad +A_2'e^{-n_2t},$$

we find

$$\left.\begin{aligned} A_1+A_2 &= \frac{E\rho}{R(R+\rho)},\\ A_1'+A_2' &= \frac{E\rho'}{R'(R'+\rho')}.\end{aligned}\right\} \quad (13)$$

Solving for A_1 A_2, we get

$$A_1=\frac{E}{R(n_1-n_2)}\left\{\frac{1}{GC}\left(\frac{\rho}{R+\rho}+\frac{\rho'}{R'+\rho'}\right)\right.$$
$$\left.+\frac{1}{R}\left(\frac{1}{RC}-\frac{n_2\rho}{R+\rho}\right)\right\}, \quad \ldots \quad (14)$$

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$$A_2 = -\frac{E}{R(n_1-n_2)}\left\{\frac{1}{GC}\left(\frac{\rho}{R+\rho}-\frac{\rho'}{R'+\rho'}\right)\right.$$
$$\left.+\frac{1}{R}\left(\frac{1}{RC}-\frac{n_1\rho}{R+\rho}\right)\right\}, \quad \ldots\ldots\ldots \quad (15)$$

and similar values for A_1' A_2'.

Hence we find

$$Gx = E\left\{\frac{R'}{R'+\rho'}-\frac{R}{R+\rho}\right\}$$
$$+\frac{E}{n_1-n_2}\left[\left(\frac{1}{R'C'}-\frac{1}{RC}\right)(e^{-n_1t}-e^{-n_2t})\right.$$
$$+\left\{\left(n_2-\frac{1}{GC}-\frac{1}{GC'}\right)e^{-n_1t}-\left(n_1-\frac{1}{GC}-\frac{1}{GC'}\right)e^{-n_2t}\right\}$$
$$\left.\times\left\{\frac{\rho}{R+\rho}-\frac{\rho'}{R'+\rho'}\right\}\right]. \quad \ldots\ldots\ldots \quad (16)$$

Let us take the case in which there is no leakage first, $\rho=\rho'=\infty$, and

$$x = \frac{E}{G(n_1-n_2)}\left(\frac{1}{R'C'}-\frac{1}{RC}\right)(e^{-n_1t}-e^{-n_2t}). \quad \ldots \quad (17)$$

Thus, if $RC=R'C'$, we see that x is zero for all values of t.

The total effect on the galvanometer, since the time of charging is short, is proportional to the quantity of electricity which passes. To find this we integrate the value of x with respect to t from 0 to τ, and suppose τ so large that

$$e^{-n_1\tau} = e^{-n_2\tau} = 0.$$

Then, if P be the quantity,

$$P = \frac{E(R'C'-RC)}{G\,RR'CC'n_1n_2};$$

also

$$n_1n_2 = \frac{G+R+R'}{GRR'CC'};$$

$$\therefore \quad P = \frac{E(R'C'-RC)}{G+R+R'}; \quad \ldots\ldots \quad (18)$$

and if H be the strength of the field in which the needle hangs, T the time of a complete oscillation, k the galvanometer-constant, and α the angle through which the needle is turned,

$$P = \frac{HT}{\pi k}\sin\frac{\alpha}{2};$$

$$\therefore \quad \sin\frac{\alpha}{2} = \frac{\pi k}{HT}\cdot\frac{E(R'C'-RC)}{(G+R+R')}, \quad \ldots \quad (19)$$

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which leads to the condition that, when there is no throw of the galvanometer, $R'C' = RC$.

We proceed to inquire what resistances will give the most accurate value for the capacity C in terms of C′, the known capacity of a standard condenser when using a given galvanometer. Let us suppose the adjustment made by varying R, and determine the error δa produced in a by an error δR in R. Then, remembering that, when the adjustment is perfect, $RC = R'C'$ and $a = 0$, if δR is the error from perfect adjustment, we have

$$\delta a = -\frac{2}{HT}\frac{E\pi k C \delta R}{(G+R+R')}; \quad \ldots \quad (20)$$

and if δC is the error in the capacity, since $C = \frac{R'C'}{R}$,

$$\delta C = -\frac{R'C'\delta R}{R^2} = \frac{HT(G+R+R')R'C'}{2E\pi k C R^2}\delta a;$$

or, since $CR = C'R'$,

$$\delta C = \frac{HT(G+R+R')}{2E\pi k R}\delta a. \quad \ldots \quad (21)$$

Now k varies as the number of turns in the galvanometer, and so also does G;

$$\therefore \quad K = \mu G,$$

$$\therefore \quad \delta C = \frac{HT\delta a}{2E\pi\mu}\left\{\frac{1}{G}+\frac{1}{R}+\frac{R'}{GR}\right\}; \quad \ldots \quad (22)$$

and if we suppose that we are liable to an error δa in a, the error in C is least when the resistances R and R′ are both high.

Thus it is best to use, with a given galvanometer, high resistances R and R′.

We arrive at the same result if we make the adjustments by varying R′ instead of R.

Again, let us suppose that we have a galvanometer with a given channel, and we wish to fill it with wire so as to be most sensitive. Let V be the volume of the channel, y the radius of the wire, l its length, ρ its specific resistance, and suppose we neglect the thickness of the covering; then

$$4\rho y^2 = V,$$

$$G = \frac{\rho l}{\pi y^2} = \frac{4\rho l^2}{V},$$

$$k = g\rho,$$

where g depends only on the form and dimensions of the

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channel. We must therefore make

$$\frac{\frac{4\rho l^2}{V}+R+R'}{l}$$

a minimum. We find

$$\frac{4\rho}{V}-\frac{R+R'}{l^2}=0; \quad \ldots\ldots \quad (23)$$

and we get finally

$$G=R+R'.$$

Now for a given value of G, R and R′ must be as high as possible; therefore we must make the resistance of our galvanometer as high as possible.

Returning to the general case in which there is a leakage in the condensers, and putting $t=\tau$, τ being so large that the terms involving $e^{-n\tau}$ may be neglected, we get

$$x=\frac{E}{G}\left\{\frac{R'}{R'+\rho'}-\frac{R}{R+\rho}\right\}. \quad \ldots\ldots \quad (24)$$

Thus there is a steady current through the galvanometer, and the needle is permanently deflected.

Again, if $t=0$, $x=0$. But let us suppose t very small, so that, on expanding e^{-nt}, powers higher than the first may be neglected, and find the initial current. We find

$$x=\frac{E}{G}\left\{\frac{R'C'-RC}{RC\,R'C'}+\frac{1}{G}\left(\frac{1}{C}+\frac{1}{C'}\right)\left(\frac{\rho}{R+\rho}-\frac{\rho'}{R'+\rho'}\right)\right\}t, \quad (25)$$

which reduces, if we neglect powers of $\frac{1}{\rho}$ above the first, to

$$x=\frac{E}{G}\left\{\frac{R'C'-RC}{RC\,R'C'}+\frac{1}{G}\left(\frac{1}{C}+\frac{1}{C'}\right)\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right)\right\}t. \quad (26)$$

By altering R the sign of this may be made to change, and thus the initial current may be in the same or in the opposite direction to the final. In practice this is indicated by a short kick of the needle in one direction, followed by a deflection in the other.

On the same assumptions as to ρ, ρ', let us find the quantity of electricity which passes through the galvanometer in time τ, τ being very short, but yet so long that $e^{-n\tau}$ may be neglected. Integrating (16) and calling the quantity P, we have

$$P=\frac{E}{G+R+R'}\left[\{R'C'-RC\}\left\{1-\frac{G\left(\frac{R}{\rho}+\frac{R'}{\rho'}\right)+RR'\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)}{G+R+R'}\right\}\right.$$
$$\left.-\left\{\frac{R'}{\rho'}-\frac{R}{\rho}\right\}\{RC+R'C'\}\right]+\frac{E}{G}\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right)\tau. \quad (27)$$

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If τ be very short, we may neglect the last term compared with the others, and, to the same degree of approximation with respect to ρ ρ, we get as the condition of no kick,

$$R'C'-RC=\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right)(R'C'+RC),$$

or

$$\frac{RC}{R'C'}=1-2\left(\frac{R'}{\rho'}-\frac{R}{\rho}\right). \quad \ldots\ldots\ldots \quad (28)$$

Again, let k, as before, be the galvanometer-constant, and δ the permanent deflexion. Then, from (25),

$$\frac{E}{G}\left\{\frac{R'}{\rho'}-\frac{R}{\rho}\right\}=k\tan\delta, \quad \ldots\ldots\ldots \quad (29)$$

$$\frac{RC}{R'C'}=1-\frac{2Gk}{E}\tan\delta; \quad \ldots\ldots\ldots \quad (30)$$

and this equation enables us to determine the capacity.

Let us suppose the adjustment made by varying R. Then, starting from a position in which the first kick is in an opposite direction to the final deflexion, adjust R until that kick is just reduced to zero, and the spot of light moves off gradually in the one direction, and after some oscillations comes to rest. Then, if δ is the deflexion of the galvanometer-needle, the capacity is

$$C=\frac{C'R'}{R}\left\{1-\frac{2Gk}{E}\tan\delta\right\}.$$

Unless the leakage is considerable, the correction will be very small.

In measuring the capacity of many condensers, the difficulty is increased by the phenomenon of electric absorption. In fact the condenser has no true capacity; for the charge produced by a given electromotive force depends on the time during which that force has acted. We may, however, take the capacity as the ratio of the instantaneous charge to the electromotive force producing it; and in this case (contact with the battery being maintained only for a very short time) we may perhaps look on electric absorption as a kind of conduction through the substance of the condenser. We must suppose that the resistance to the conduction is a function of the time, which becomes indefinitely great after a not very long interval, but which we may perhaps treat as sensibly constant during the time for which contact is maintained; and if ρ_0, ρ'_0 be the values of this resistance during that

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interval, and, as before, we may neglect $\frac{1}{\rho_0^2}$ &c. and higher powers, we have, as the value of the capacity,

$$C=\frac{R'C'}{R}\left\{1-2\left(\frac{R'}{\rho'_0}-\frac{R}{\rho_0}\right)\right\}.$$

Thus a small correction should be applied to the value $\frac{R'C'}{R}$, depending on the rate of absorption during the interval for which contact is maintained with the battery. An approximation to this quantity may be obtained by charging the condenser for some time with a battery of known electromotive force, and then allowing it to discharge itself at small intervals of time through the galvanometer. On the whole, however, the results of measurements made, neglecting this correction, are fairly satisfactory.

The capacity of a paraffin condenser was determined by several observers during the past term at the Cavendish Laboratory. Their results differed by from $\frac{2}{3}$ to 1 per cent. The standard used was not in all cases the same; and the measures obtained by one observer, comparing this same condenser with two different standards, differed by about $\frac{1}{3}$ per cent. It seemed possible to determine within 10 ohms, when each of the resistances R, R′ was about 5000 ohms, the value of R for which the initial kick was zero.

L. *On a convenient Term expressing the Change from Radiant Heat and Light into Sound.* By Ernest H. Cook, *B.Sc., F.C.S.**

WHILE making his experiments which resulted in the production of the photophone, Prof. Graham Bell made the discovery that, when rays of light fall upon certain substances, the molecules of those substances are thereby caused to vibrate in such a manner as to give rise to sound†. The mutual convertibility of the undulations which produce light and those which produce sound was thus clearly established. Thinking these sounds to be produced by rapid changes of temperature in the body impinged upon, Prof. Tyndall made a series of experiments in order to determine the different powers of bodies to absorb heat. Operating with an intermittent beam from the electric lamp, and certain

* Communicated by the Author.

† Lecture at the Boston Meeting of the American Association for the Advancement of Science, August 1880.

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gases and vapours enclosed in sealed tubes, he was able to produce musical tones from the substances*. It was thus proved that vaporous bodies as well as solids were able to convert thermal undulations into sonorous ones. Other experimenters have extended these observations†. The important result of these experiments is *that the molecules of certain bodies possess the power of transforming the undulations of short period which fall upon them into those of long period.* Expressed briefly, some bodies are capable of transforming inaudible undulations into audible ones. Now refractive bodies have the power of transforming non-luminous calorific rays into luminous ones; and this passage from the one state into the other is called *calorescence.* Again, when the ultra-violet rays are permitted to fall upon certain substances they render the substance luminous. In other words, the substance has transformed the non-luminous undulations into luminous ones. To this change the term *fluorescence* has been applied. Thus we have:—

Fluorescence—change from more rapid into less rapid undulations.

Calorescence—change from less rapid into more rapid undulations.

In the new phenomena we have again a change from more rapid into less rapid, which is very similar to that which occurs in fluorescence. To the change which takes place in this case, therefore, I venture to propose the term "*Sonorescence.*" A body such as hard rubber, in which it was first noticed, would be called a *sonorescent* body, just as sulphate of quinine is a fluorescent body. The points of similarity between the three phenomena are sufficiently evident; the chief points of difference are the following:—

(1) The change in sonorescence is greater in magnitude and is different in kind from that in the other two cases. In fluorescence and calorescence the particles oscillate in every case across the direction of propagation, but in sound we believe them to move *in the* direction of propagation. Thus, in the new phenomena we have a change of direction and also a change in frequency of the oscillations.

(2) In fluorescence and calorescence the result is in both cases to produce *luminous* undulations from *non-luminous* ones. In sonorescence, on the contrary, we produce sonorous

* "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter," read before the Royal Society, Jan. 13, 1881.

† More especially ought Prof. W. C. Röntgen to be mentioned. This gentleman seems to have anticipated some of Tyndall's identical experiments. See Phil. Mag. April 1881.

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undulations from luminous ones. The first portion of the word proposed, however, has so decided a meaning that no one can imagine it to mean a production of luminous undulations, but will immediately recognize it as meaning the production of sonorous ones.

Bristol, April 4, 1881.

LI. *On Action at a Distance.* *By* WALTER R. BROWNE, *M.A., M. Inst. C.E., late Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM indebted to your courtesy for space to conclude (at least as far as I am concerned) the controversy which has arisen out of my paper on Action at a Distance. I will confine myself to a very few words as to the points at issue between myself and Dr. Lodge.

The difficulty of understanding each other's position has arisen from our employing the word Work in different senses. Dr. Lodge, following modern works on Dynamics, defines Work done as equivalent to Energy exerted, and as measured by the product of the moving force and the distance through which its point of application is moved. I, looking at the matter from the point of view of Applied Mechanics, followed the definition of Rankine, according to which Work is done only when there is a resisting force acting in the opposite direction to the moving force; and is measured by the product of this resisting force and the distance through which its point of application is moved*. I perhaps owe Dr. Lodge an apology for not having seen this from the first; but I will not dwell longer on this or any other matters of mere personal controversy between us.

It remains, finally, to give my reasons for rejecting the remarkably short and simple argument by which Dr. Lodge conceives that he has disposed for ever of the idea of Action at a Distance. This argument† is as follows:—It first states generally the law of the equality of Action and Reaction, where two bodies, A and B, act "directly and solely" on each other with forces F, through distances s; and then asserts that,

* For this definition, and for a proof that Rankine did not include in his Resistances the inertia of the body, I may refer to the following articles in his 'Applied Mechanics,' first edition, 1858:—page 477, arts. 511-514; page 499, art. 549; and especially page 622, art. 689.

† Phil. Mag. January 1881, p. 36.

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by the law of the Conservation of Energy, the two "Works," or the two products of force and distance moved through, must be equal and opposite. It then proceeds:—"Since the forces (F) are equal and opposite, and the works (Fs) are also equal and opposite, the distances (s) must be equal but not opposite; that is, the two bodies must move over precisely the same distance and in the same sense; which practically asserts that they move together and are in contact, as long as the action is going on."

This argument may be confuted in several ways.

(a) Force is measured by the acceleration, or change of velocity, which it generates; and if the force F, with which A acts on B, is equal and opposite to the force $-$F, with which B acts on A, it follows (assuming the masses to be equal) that the acceleration of B will be equal and opposite to the acceleration of A. Hence, even if A and B are moving "together and in contact" at the beginning of the space s, they will not be so at the end, because their velocities will have changed.

(b) The argument is built on the assumption that if one of the works is taken as positive, say W, the other will be negative, say $-$W. But Work, on Dr. Lodge's definition, or Energy, on Rankine's definition, may be measured by *vis viva*, or by $\frac{m}{2}v^2$, and therefore is an essentially positive quantity (as pointed out by Rankine, 'Applied Mechanics,' art. 548, p. 499). We can no more have negative work than we can have negative mass.

(c) There is no difficulty in adducing a case in which the proposition is obviously untrue. This is the simple one of two equal particles meeting each other with equal and opposite velocities. It will be admitted that both will be reduced to rest, and that, until they have been reduced to rest, they are not moving "over the same distance and in the same sense." Therefore, according to Dr. Lodge, until they have been reduced to rest they cannot exercise any action upon each other. What, then, is the action which reduces them to rest? To say they are reduced to rest instantaneously on coming into absolute contact, does not remove the difficulty; and if it did, it would be by assuming that the force acting between them was literally *infinite*, since it would stop a finite velocity in no space at all.

(d) Finally, I may point out what is the real and intrinsic vice of Dr. Lodge's argument, viz. that he assumes that the Conservation of Energy is universally true of Potential Energy only, whereas (as is shown in any text-book) it is only universally true of Kinetic and Potential Energy together. This

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appears at once on putting his case in mathematical form. He supposes two particles, m and m', moving in the same straight line (say the axis of x) with velocities $\frac{dx}{dt}$ and $\frac{dx'}{dt}$, and acting on each other with equal and opposite moving forces F. Then the Force Function, during any time δt, is

$$\mathrm{F}\frac{dx}{dt}\delta t - \mathrm{F}\frac{dx'}{dt}\delta t.$$

According to Dr. Lodge, this is always = 0. But, as is well known, the true equation is

$$\mathrm{F}\frac{dx}{dt}\delta t - \mathrm{F}\frac{dx'}{dt}\delta t = m\frac{dx}{dt}\frac{d}{dt}\left(\frac{dx}{dt}\right)\delta t - m'\frac{dx'}{dt}\frac{d}{dt}\left(\frac{dx'}{dt}\right)\delta t\,;$$

where the left-hand side, taken negatively, is the Potential Energy, and the right-hand side is the change in the *vis viva* or Kinetic Energy of the system. This right-hand side clearly varies according to the values of $\frac{dx}{dt}$, $\frac{dx'}{dt}$, or of the velocities. *If these velocities are equal* (assuming the masses also equal), *then* the right-hand side vanishes, and we have as a particular case that which Dr. Lodge assumes to hold universally. What *does* hold universally is that the sum of the two sides (*i. e.* of the Potential Energy and the Kinetic Energy) is constant; and this, and this only, is the principle known as the Conservation of Energy.

WALTER R. BROWNE.

LII. *On the Physical Units of Nature.* *By* G. JOHNSTONE STONEY, *D.Sc., F.R.S., Vice-President of the Royal Dublin Society**.

1. WHEN mathematicians apply the sciences of measurement to the investigation of Nature, they find it convenient to select such units of the several kinds of quantity with which they have to deal as will get rid of any coefficients in their equations which it is possible in this way to avoid. Every advance in our knowledge of Nature enables us to see more distinctly that it would contribute to our further progress if we could effect this simplification, not only with reference to certain classes of phenomena, but throughout the whole domain of Nature.

* From the 'Scientific Proceedings' of the Royal Dublin Society of February 16, 1881, being a paper which had been read before Section A of the British Association, at the Belfast Meeting in 1874. Communicated by the Author.

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2. Hitherto the practice of mathematicians has been governed by the demands of the science of mechanics, in the greater part (though not in the whole) of which science it is possible to derive the units of all the other kinds of quantity from any three which may be chosen. A system built in this way upon a foundation which is arbitrarily assumed is necessarily an artificial system. The units which are usually selected as the fundamental units of a series of arbitrary systematic measures are:—

the metre* for lengthine, or unit of length;
the gramme for massine, or unit of mass; and
the solar second for timine, or unit of time.

These three, and all the units which may be derived from them, may be called the metric series of units; and in this investigation they will be represented by small letters. Thus, *the fundamental metric units being*

l_1, the metre, the metric lengthine, or unit of length;
t_1, the solar second, the metric timine, or unit of time;
m_1, the gramme, the metric massine, or unit of mass;

some of the derived units will be:—

v_1, the metric velocitine, or unit of velocity, which is a velocity of one metre per second;
f_1, the metric forcine, or unit of force, which is the hyper-decigramme†—this being the force which, if it acted in a fixed direction on a mass of one gramme for a second,

* Since this paper was written the centimetre has been suggested as a unit of length, and has been largely made use of.

† The hyper-decigramme means the gravitation or downward force towards the earth of a mass which exceeds a decigramme in the ratio of $\frac{10}{g}$, where g is the acceleration of gravity measured in metres per second per second. The appropriateness of the term hyper-decigramme arises from the circumstance that the coefficient $\frac{10}{g}$ everywhere exceeds unity, whether within the earth, outside it, or on its surface; and the convenience of the term arises from the circumstance that *on the earth's surface* the coefficient nowhere exceeds unity by more than a small fraction, so that the hyper-decigramme is a force which but little differs in value from that gravitation or weight of a decigramme with which we, inhabitants of the earth, have become familiar; so that the name suggests to us the *amount* of the force. Gravitation is the downward force, and gravity is the downward acceleration towards the earth as *observed*. They are chiefly due to the attraction of the earth, and in a small degree, when the observation is made on or within the earth, to the earth's rotation. This is the meaning of the word gravity as it is used by the classical writers on mechanics (see Pouillet's *Mécanique*, *passim*); and the practice of some modern writers, who use this term to designate a force instead of an acceleration, is to be deprecated.

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would in that time change its velocity by one metre per second;

m_1, the metric unit of the coefficient in the expression $\mu\frac{mm'}{r^2}$ for the gravitation of two masses towards one another: this unit is the coefficient which should be used if each gramme mass attracted other ponderable matter at a metre distance with such intensity as would impress on it an acceleration towards the attracting gramme of one metre per second per second.

e_1, the electromagnetic electrine, or the electromagnetic unit quantity of electricity in the metric series, is that quantity of each of the two kinds of electricity which must be discharged every second in opposite directions along a wire in order to maintain in it the metric unit current,—this currentine or unit current being defined as the current which must exist in a wire a metre long in order that it may exert a force of a hyper-decigramme on ponderable matter at a metre distance charged with a unit of magnetism; and the unit charge of magnetism of either kind being defined as that quantity which, acting on ponderable matter at a metre distance, charged with an equal quantity of magnetism, exerts on it the unit force—that is, one hyper-decigramme.

3. It is easy to ascertain the relation of this metric electrine to the B.A. (British-Association) standards for electrical measurement, which are those most in use. The B.A. units are electromagnetic units based on the following fundamental units—the second for unit of time, the metre-seven (the quadrant of the earth, or 10^7 metres) for unit of length, and the eleventh-gramme (or gramme divided by 10^{11}) for unit of mass. These were so chosen as to furnish a connected body of systematic units with such values that the practical electrician could conveniently use them. Now the "dimension" of electromagnetic quantity of electricity is $[L\sqrt{LM}]$ (see B.A. Report for 1863, p. 159)*. Hence, and from the foregoing values of the lengthine and massine of the B.A. series,

$$e_1 : \text{one Ampère} = 1 : \sqrt{\frac{10^7}{10^{11}}};$$

therefore $e_1 = 100$ Ampères.

* This follows at once from the fundamental equations of electromagnetism, viz.:—

$$F \propto \frac{EE^1}{r^2};\quad E = Ct;\quad F = \frac{CIM}{r^2};\quad F = \frac{MM^1}{r^2}.$$

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The term Ampère is here used to designate the B.A. unit of quantity corresponding to the ohm (the B.A. electromagnetic unit of resistance), the volt (the corresponding unit of electromotive force), the weber (unit of current), and the farad (unit of capacity). The *electrostatic* units of the B.A. series might with great advantage be called the static-ampère, static-ohm, static-volt, and static-farad.

4. Units like the above, whether of the metric or of the B.A. series, of which three are fundamental, and all others derived from them in such a way as will exclude unnecessary coefficients from our equations, are called systematic units. In forming the existing artificial series of systematic units, it has been usual to regard the units of length, time, and mass as fundamental, and the rest as derived; but *there is nothing to prevent our regarding any three independent members of the series as fundamental, and deriving the others from them.* It is the aim of the present paper to point out that Nature presents us with three such units; and that if we take these as our fundamental units instead of choosing them arbitrarily, we shall bring our quantitative expressions into a more convenient, and doubtless into a more intimate, relation with Nature as it actually exists. I will then approximate to the values of the units of length, time, and mass belonging to this, which is a truly natural series of physical units.

5. For such a purpose we must select phenomena that prevail throughout the whole of Nature, and are not specially associated with individual bodies. The first of Nature's quantities of absolute magnitude to which I will invite attention is that remarkable velocity of an absolute amount, independent of the units in which it is measured, which connects all systematic electrostatic units with the electromagnetic units of the same series. I shall call this velocity V_1. If it were taken as our unit velocity, we should at one stroke have an immense simplification introduced into our treatment of the whole range of electric phenomena, and probably into our study of light and heat.

Again, Nature presents us with one particular coefficient of gravitation, of an absolute amount independent of the units in which it is measured, and which appears to extend to ponderable matter of every description throughout the whole material universe. This coefficient I shall call G_1. If we were to take this as our unit of coefficients of attraction, it is presumable that we might thereby lay the foundation for detecting wherein lies the connexion which we cannot but suspect between this most wonderful property common to all ponderable matter and the other phenomena of nature.

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And, finally, Nature presents us, in the phenomenon of electrolysis, with a single definite quantity of electricity which is independent of the particular bodies acted on. To make this clear I shall express "Faraday's Law" in the following terms, which, as I shall show, will give it precision, viz.:—*For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte, which is the same in all cases.* This definite quantity of electricity I shall call E_1. If we make this our unit quantity of electricity, we shall probably have made a very important step in our study of molecular phenomena.

Hence we have very good reason to suppose that in V_1, G_1, and E_1 we have three of a series of systematic units that in an eminent sense are the units of nature, and stand in an intimate relation with the work which goes on in her mighty laboratory.

6. The approximate values of V_1 and G_1 are known; and I will presently endeavour to evaluate E_1. V_1 has been variously determined by experiment as 3·10 metre-eights per second, 2·82 metre-eights per second, 2·88 metre-eights per second, and may be assumed to be not far from 3 metre-eights per second. Accordingly we may put

$$V_1 = 3\ \text{VIII metres per second.} \quad . \quad . \quad . \quad (1)$$

Similarly, if we use the value given by Sir John Herschel for the mass of the earth, viz. 5942 XVIII* English tons, which = XXIV grammes, we find that

$$G_1 = \frac{2}{3}\,\frac{1}{\text{XIII}}[\mu]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

7. To determine E_1, we must first establish a relation between the gaseous molecule of a body and what in chemistry is called its atom. To do this, let us start with the definition that a *chemical atom* is the smallest mass of each kind of ponderable matter that *has been found* to enter or leave a combination. Now, from Boyle and Charles's law we know that in all gases there are approximately the same number of molecules per litre, if they be taken at the same temperature and pressure; from experiments on diffusion we know that these molecules are alike in mass; and from the phenomena of chemistry we know that they are alike in other respects.

* The Roman figures following a number stand for cyphers. Thus, 3 VIII signifies 3×10^8, and 5942 XVIII stands for eighteen cyphers following 5942. Where no number precedes the Roman figures the number 1 is to be understood; so that in XXIV grammes, the Roman figures stand for 1 followed by 24 cyphers, in other words, for 10^{24}, a number which may conveniently be called the unit-twenty-four.

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Let, then, a litre of hydrogen and a litre of chlorine be mixed and exploded, and let the resulting hydrochloric-acid gas be brought back to the original temperature and pressure. It is then found to measure two litres. Hence, if N be the number of molecules in a litre of gas at that temperature and pressure, we learn by this experiment that N molecules of hydrogen and N molecules of chlorine produce 2N molecules of hydrochloric acid. Hence, and since the molecules within each gas are alike, each molecule of hydrochloric acid must contain the quantity of hydrogen represented by a semi-molecule of hydrogen gas and the quantity of chlorine represented by a semi-molecule of chlorine gas. We are thus introduced to the semi-molecule of each of these gases as a quantity which enters into combination; and as no other experiments suggest a smaller quantity, the semi-molecule of hydrogen and the semi-molecule of chlorine are, in the present state of science, to be accepted as the chemical atoms of these substances. Hence we may write

H, the atom of hydrogen = the semi- gaseous molecule of hydrogen; and

Cl, the atom of chlorine = the semi- gaseous molecule of chlorine;

and we see that HCl is the proper formula for hydrochloric acid. We may further deduce from the observed densities of the gases, that the masses of the atoms of hydrogen, chlorine, and hydrochloric acid are to one another in the ratio of 1, $35\frac{1}{2}$, $36\frac{1}{2}$.

Another experiment shows us that a litre of steam may be resolved into a litre of hydrogen and half a litre of oxygen at the same temperature and pressure—in other words, that N molecules of steam are formed of N molecules of hydrogen and $\frac{N}{2}$ molecules of oxygen. Hence each molecule of steam contains a whole molecule (or two atoms) of hydrogen and a semi-molecule of oxygen. We thus arrive at the semi-molecule of oxygen as a quantity that enters into combination; and as all other experiments with oxygen concur, the semi-molecule of oxygen is to be received as its atom, and H_2O is the proper formula for what is both the gaseous molecule and the atom of water. From the densities we may also deduce that 16 is the atomic weight of oxygen, *i. e.* that an atom of oxygen is sixteen times as heavy as an atom of hydrogen.

Similarly from the densities of ammonia and of its constituents, we learn that the atom of nitrogen is the semi-molecule, and that the mass of its atom is fourteen times that of hydrogen.

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It must not be assumed that the atom is always the semi-molecule. In some cases it is found to be the entire molecule, and in other cases the quarter molecule. Thus the mercuric compounds of mercury give vapours of the same bulk as the vapour of the mercury they contain, and indicate an atom of mercury equal to its molecule; while the other volatile compounds of mercury contain more than one molecule of mercury in each molecule of the compound, and therefore do not disturb this conclusion. Again, a litre of phosphuretted hydrogen yields a quarter of a litre of the vapour of phosphorus and one and a half litre of hydrogen, indicating that the quarter molecule of phosphorus is its atom. The same is true of arsenic.

A similar treatment of marsh-gas furnishes 12 as the mass of an atom of carbon, although carbon is not sufficiently volatile to enable us to ascertain the relation of its atom to its gaseous molecule.

By extending this method to all the available cases, we may deduce *from the fundamental properties of gases* a demonstration of a great part of the modern Table of atomic weights and of the doctrine of atomicity which depends on it. Thus, two bonds* are necessary to connect the group SO_4 with the two atoms of hydrogen that are united to it in sulphuric acid, while one bond is sufficient to join the atoms of hydrogen and chlorine in an atom of hydrochloric acid, and so in other cases.

8. Now the whole of the quantitative facts of electrolysis may be summed up in the statement that A DEFINITE QUANTITY OF ELECTRICITY TRAVERSES THE SOLUTION FOR EACH BOND THAT IS SEPARATED. Thus, if a current pass in succession through vessels containing solutions of sulphuric acid and hydrochloric acid, two atoms of hydrochloric acid will be decomposed in the one vessel for each atom of sulphuric acid that is decomposed in the other; but *the number of bonds separated will be the same in both vessels.*

9. It is the quantity of electricity that passes per bond separated that we have now to determine, and this may be done approximately in the following manner. Several inquiries (see Prof. J. Loschmidt, "Zur Grösse der Luftmolecüle," Academy of Vienna, Oct. 1865; G. Johnstone Stoney, "On the Internal Motions of Gases," Phil. Mag. August 1868; and Sir William Thomson, "On the Size of Atoms," 'Nature,' March 31, 1870) have led up to the conclusion that the number of molecules in each cubic millimetre of a gas at

* The word bond is here used of the *connexions* between atoms when they enter into combination. When we use this, which seems the proper signification of the word, the bonds are to be distinguished from the hands or feelers which each atom has, and which, by grappling with the hands or feelers of other atoms, establish bonds between them.

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atmospheric temperatures and pressures is somewhere about a unit-eighteen (10^{18}). Hence the number of molecules in a litre will be about a unit-XXIV. Now, a litre of hydrogen at atmospheric pressures and temperatures weighs, roughly speaking, a decigramme. Hence the mass of each molecule of hydrogen is a quantity of the same order as a decigramme divided by a unit-XXIV, *i. e.* a XXVth gramme. The chemical atom is half of this. Hence the mass of a chemical atom of hydrogen may be taken to be somewhere about half a twenty-fifth-gramme. There is no advantage in retaining the coefficient half in an estimate in which we are not even sure that we have assigned the correct power of 10; and I will therefore, for the sake of simplicity, take the XXVth gramme as being such an approach as we can attempt to the value of the mass of an atom of hydrogen.

10. Now, it has been ascertained by experiment that, for every ampère of electricity that passes, ninety-two sixth-grammes (*i. e.* ninety-two millionths of a gramme of water) are decomposed (see Brit.-Assoc. Report, 1863, p. 160). This water is the result of a secondary action in the voltameter; but that does not affect the present inquiry. Ninety-two VIth grammes of water contain about one Vth gramme of hydrogen, which is therefore the quantity evolved. The metric unit of electricity e_1 is 100 ampères, and will therefore set free 100 Vth grammes of hydrogen, *i. e.* one milligramme. Now it appears, from the last paragraph, that this quantity of hydrogen contains $\frac{\text{XXV}}{1000}$ atoms, *i. e.* XXII atoms. And as there is a bond ruptured for each atom of hydrogen set free, this is also the number of bonds broken; in other words, the quantity of electricity corresponding to each chemical bond separated is

$$\mathrm{E}_1 = \frac{1}{\text{XXII}}\, e_1. \quad \ldots\ldots\ldots \quad (3)$$

Collecting our numerical results, they are

$$\mathrm{V}_1 = 3\ \text{VIII metres per second}, \quad \ldots \quad (1)$$

$$\mathrm{G}_1 = \frac{2}{3}\,\frac{\mu_1}{\text{XIII}}, \quad \ldots\ldots\ldots \quad (2)$$

$$\mathrm{E}_1 = \frac{e_1}{\text{XXII}}, \quad \ldots\ldots\ldots \quad (3)$$

$$= \frac{1}{\text{XX}}\ \text{ampère}.$$

We have thus obtained approximate values in known measures for the three great fundamental units offered to us by Nature, upon which may be built an entire series of physical

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units deserving of the title of a truly Natural Series of Physical Units.

11. It now only remains to deduce the units of length, time, and mass belonging to this series. For this purpose we may use dimensional equations. Remembering, as is well known, that the dimension of a unit of velocity is $\left[\frac{L}{T}\right]$, that of a unit of coefficients of attraction $\left[\frac{L^3}{MT^2}\right]$, and that of an electromagnetic unit of quantity $[\sqrt{LM}]$, we find from equations (1), (2), and (3) respectively that

$$\frac{L_1}{T_1} = A\frac{l_1}{t_1}, \quad \ldots\ldots \quad (4)$$

$$\frac{L_1^3}{M_1 T_1^2} = B\frac{l_1}{m_1 t_1^2}, \quad \ldots\ldots \quad (5)$$

$$\sqrt{L_1 M_1} = C\sqrt{l_1 m_1}; \quad \ldots\ldots \quad (6)$$

in which L_1, M_1, and T_1 are used to designate the units of length, mass, and time in the "Natural" series; while l_1, m_1, and t_1 represent the corresponding units in the metric series, viz. the metre, gramme, and second. A, B, and C also are used, for brevity, to stand for the numerical coefficients of equations (1), (2), and (3); viz. for the numbers 3 VIII, $\frac{2}{3}\frac{1}{\text{XIII}}$, and $\frac{1}{\text{XXII}}$.

Solving equations (4), (5), and (6), we find

$$L_1 = \frac{C\sqrt{B}}{A} l_1, \quad \ldots\ldots \quad (7)$$

$$T_1 = \frac{C\sqrt{B}}{A^2} t_1, \quad \ldots\ldots \quad (8)$$

$$M_1 = \frac{CA}{\sqrt{B}} m_1. \quad \ldots\ldots \quad (9)$$

Substituting for A and B their numerical values, and writing metre, second, and gramme for l_1, t_1, m_1,

$$L_1 = C \; \frac{1}{3\sqrt{15}} \frac{1}{\text{XIV}} \text{ metres},$$

$$T_1 = C\frac{1}{3} \frac{1}{3\sqrt{15}} \frac{1}{\text{XXII}} \text{ seconds},$$

$$M_1 = C \; 3\sqrt{15} \; \text{XIV grammes};$$

or, more simply (inasmuch as 10 is sufficiently near to $3\sqrt{15}$ to be used instead of it in an approximation like the present),

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$$L_1 = C \frac{1}{XV} \text{ metres}, \quad . \quad . \quad . \quad . \quad . \quad (10)$$

$$T_1 = C \frac{1}{3} \frac{1}{XXIII} \text{ seconds}, \quad . \quad . \quad . \quad (11)$$

$$M_1 = C\ XV \text{ grammes}. \quad . \quad . \quad . \quad . \quad (12)$$

In obtaining these equations we have only used the numerical values of V_1 and G_1, which are known to a satisfactory degree of approximation; and if we go no further, there will remain but one arbitrary member in the entire of the resulting series of systematic physical units.

12. If we also introduce the numerical value found above for C, which depends on E_1 and is less accurately known, we obtain the following actual values for these units of Nature:—

$$L_1 = \frac{1}{XXXVII} \text{ of a metre}; \quad . \quad . \quad . \quad (13)$$

$$T_1 = \frac{1}{3} \frac{1}{XLV} \text{ of a second}; \quad . \quad . \quad . \quad (14)$$

$$M_1 = \frac{1}{VII} \text{ of a gramme}. \quad . \quad . \quad . \quad (15)$$

Or, in other words—

The natural unit of length approaches in value to the thirty-seventh metre (*i. e.* the metre divided by 10^{37}).

The natural unit of time approaches in value to one third of the forty-fifth second (*i. e.* one third of the second of time divided by 10^{45}); and

The natural unit of mass approaches to the seventh gramme (*i. e.* the gramme divided by 10^{7}).

13. This appears the best attempt we can yet make to determine these remarkable units. In the series to which they belong all the electrostatic units will be identical with the corresponding electromagnetic units, all the forces of Nature that are known to obey the law of the inverse square, whether they arise from gravitation, electricity, or magnetism, will be expressed without coefficients, and the chemical bond, which seems to be the unit of concrete Nature, is brought into its proper relation to physics.

Postscript.—Many persons find it difficult to conceive of G_1 as a unit. G_1 may be avoided and M_1 be substituted for it, if M_1 be defined as a mass such that it attracts an equal mass at a distance with the same force with which two units of electricity, as defined above in section 5 (*i. e.* each equal to E_1), would, if placed at the same distance asunder, act on each other. The three fundamental units of the Natural System will then be V_1, E_1, and M_1, from which all others are to be derived. This M_1 is the same as the M_1 of sections 11 and 12.

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LIII. *On the Importance of Experiments in relation to the Mechanical Theory of Gravitation.* *By* S. Tolver Preston*.

THE assumption of "action at a distance" has notoriously tended to check experimental inquiry in relation to the mechanical cause of gravitation, as it has hindered investigation in other branches of physics. At one time, for instance, on the basis of this theory of "action at a distance," electric action was supposed to propagate itself in some mysterious way across space without the intervention of matter; so that all substances were assumed to play a perfectly neutral part in the passage of the electric action—until Faraday, rising above the prejudices of his time, put this question under the test of experiment, and by his discovery of a "specific inductive capacity," demonstrated the supposition thus formed to be false. Yet so great was the strength of preconceived opinion, that notoriously Faraday's discovery was not fully accepted until years afterwards, when its practical application to telegraph-cables forced it on the attention of electricians.

So now in the present day, in spite of the continual demolition of spiritualistic views (*i. e.* views of action without the intervention of matter) by advancing science, the *modus operandi* of gravitation remains the one outstanding subject for doubt; and it appears to be assumed by many (much in analogy with the former question of "specific inductive capacity") that experiments with the view to decide whether any minute specific differences might exist in the effects of gravity, dependent on structure, form, &c. of substances, would be out of place—gravity being rather considered as something *not* physical at all (although admitted, as it were, by courtesy into "*physical*" science).

No doubt it may be agreed at the outset here that such specific differences in gravific effects (if they exist) must be very small, or they would have been accidentally detected. But the history of science shows that phenomena are often not detected by accident, but have to be carefully searched for. Where would the discovery of diamagnetism (for example) have been without a careful investigation? Here it was generally assumed beforehand that, with the exception of certain isolated magnetic substances (iron &c.), no specific differences would exist in the behaviour of bodies in general to magnetism (which was, in the same way, supposed to be an occult kind of "action at a distance"), until Faraday, again resorting to rigorous experiment, broke down once more this belief, and that only after the most careful and laborious investigations.

* Communicated by the Author.

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May not analogous considerations apply to gravity (still regarded by some as the result of a mystic "action at a distance"), and which, as an actual fact, now remains as the last remnant of spiritualism to be expelled from physics. In relation to this subject, Sir William Thomson, in an important development of Le Sage's theory of gravitation, published in the Philosophical Magazine for May 1873, makes the following suggestive remark, viz. that, assuming certain possible conditions to exist, "crystals would generally have different permeabilities* in different directions, and would therefore have different weights according to the direction of their axes relatively to the direction of gravity. No such difference has been discovered; and it is certain that, if there is any, it is extremely small. Hence the constituent atoms, if æolotropic as to permeability, must be so but to an exceedingly small degree" (page 331)†.

Might not this point be worthy of experiment? Although the discovery of some such minute specific differences would not, in point of principle, be necessary to the establishment of the truth of the *existence* of an explanation for gravitation, yet a discovery of this kind would be highly interesting as a confirmation of the truth of the particular explanation afforded by the *kinetic* theory of gases (which depends on the permeability of gross matter by a gaseous æther, whose minute component particles have an extremely long mean path). The groundwork of this explanation (*i. e.* the one first started in very crude form by Le Sage, of Geneva) is now, it may be observed, generally regarded by competent judges as the only *conceivable* rough basis on which an intelligible mechanical theory of gravitation admits of being built.

The main object of this paper is to call more particular attention to the desirability of an experimental search in relation to the mechanical theory of gravitation, or to the expediency of a more precise and exact observation of gravific effects under diverse conditions of form, structure, &c. of

* It should be kept in view, that one of the essential conditions of Le Sage's theory is that gross matter should be permeable by the minute particles of a finer material, or æther.

† Sir William Thomson remarks further as follows in relation to this point, viz.:—"A body having different permeabilities in different directions would, if of manageable dimensions, give us a means for drawing energy from the inexhaustible stores laid up in the ultramundane corpuscles—thus: first, turn the body into a position of maximum weight; secondly, lift it through any height; thirdly, turn it into a position of maximum weight; fourthly, let it down to its primitive level. It is easily seen that the first and third of those operations are performed without the expenditure of work; and, on the whole, work is done by gravity in operations 2 and 4" (page 331).

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bodies, with a view to determine if certain small specific variations may not have escaped notice, owing to their *not having been searched for*, on account of the bias of preconceived ideas, and the lethargy produced by the influence of preformed theories. Crystalline bodies of birefracting properties &c. which, in relation to the luminiferous æther, are permeable to light with different velocities in different directions, may specially suggest themselves for experiment.

Since the successful pursuit of a research of this kind, where specific differences of a very minute character are in question, would no doubt entail considerable experimental resources, with refined and delicate apparatus, which might not be at the disposal of every one, the more therefore does it become desirable to attract general attention to the subject. The comparative dearth of discovery of any great physical principle (of a fundamental character at least) within the last few years would seem, if any thing, to call for additional zeal in experimental enterprise. Although a negative result to the investigations would not be decisive one way or the other in regard to the validity of the explanation of gravity afforded by the kinetic theory (for, of course, it is well conceivable that such specific variations in gravific effect, if they exist, might be too minute for detection with the appliances at our command), yet a positive result would be so highly interesting and important, that even a small prospect of success would amply repay the labour of a careful trial, and enlist usefully the skill and ingenuity of experimenters.

Heatherfield, Bournemouth,
March 2, 1881.

LIV. *Intelligence and Miscellaneous Articles.*

THE EFFECT OF GREAT COLD UPON MAGNETISM.

BY JOHN TROWBRIDGE.

AN investigation upon the magnetic condition of steel and upon the magnetic permeability of iron is now in progress in the Physical Laboratory of Harvard University. The preliminary experiments are interesting, since they show that very low temperatures exercise far greater influence on the magnetic condition than has been noticed by previous observers.

It is stated by Wiedemann* that the cooling below the temperature at which steel is magnetized enfeebles the magnetic condition. A bar which was magnetized at 6° C. or 8° C. gave at 4° C. and −25° C. intensities represented by 5·08 and 4·90. This represents a loss of less than 4 per cent. In my experiments the magnetic bar magnetized at 20° C., when subjected to a tempera-

* Daguin, *Traité de Physique*, nouv. ed., "Influence de la température d'aimantation."

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ture of about −60° C., loses a far greater percentage of its magnetism. In one case a bar magnetized to saturation lost 66 per cent. of its magnetism.

The low temperature was produced by solid carbonic acid and ether; and the magnetic moments of the bar were measured by placing it east and west of a suspended magnet which was provided with a mirror. In this case we have the magnetic moment

$$M = \frac{1}{2} r^3 T \tan \phi,$$

where r = distance of magnet, T = horizontal intensity of earth's magnetism, and ϕ = angle of deflection of suspended magnet. The angles were observed before the magnetic bar was surrounded with the freezing-mixture and afterwards at intervals when it was subjected, without removing it from its first position, to the influence of the carbonic acid. The following Table shows the variations of the deflections to which the magnetic moments are proportional.

When subjected to Freezing-mixture.

Before freezing.	½ min. observ.	After interval of 3½ min. ½ min. obs.	1 min. observ.	2 min. observ.	5 min. observ.
·6390	·6050	·5860	·5820	·5790	·5540
·6395	·6020	·5850	·5815	·5740	·5415
·6390	·6000	·5840	·5825	·5700	·5480
	·5980	·5840	·5815	·5650	·5480
	·5960	·5830		·5600	
	·5950	·5820			
	·5940				
	·5930				
	·5920				
	·5910				
	·5900				
	·5890				

The zero of the scale was ·5000; and the observations are expressed in fractions of a metre. It will be seen that this bar lost in forty-seven minutes nearly two thirds of its original magnetic condition. After twenty-four hours' exposure to the temperature at which it had been magnetized, its magnetic condition was 50 per cent. of its original state.

A ring of soft iron was next experimented upon according to the method of Professor Rowland; and it was found that its magnetic permeability, on being subjected to very low temperature, differed greatly from the results obtained for soft iron at ordinary temperatures.

It is well stated by Dr. V. Strouhal and Dr. C. Barus, in a paper on the Physical Condition of Steel, *Ann. der Physik und Chemie*, 1880, No. 13, that we must regard each bar of steel, in regard to its magnetic condition, as an individual of special characteristics; and a long investigation will be necessary to determine the limits of the effect of cold upon magnetism.—Silliman's *American Journal of Science*, April 1881.

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ON THE GALVANIC CONDUCTIVITY OF HEATED GASES.

BY M. R. BLONDLOT.

Gases are generally considered incapable of transmitting the current furnished by a pile consisting of only a few elements. A single exception has been pointed out by M. E. Becquerel*—that of gases brought to red heat. M. Becquerel ascertained that at this high temperature different gases, among others air, permit the current of a pile to pass, and even that of a single Bunsen element. He found, however, that the resistance of gases followed very different laws from those which have been established for solids and liquids: it depends on the intensity of the current, the number of the pile-elements, and, between two electrodes with unequal surfaces, on the direction of the current. The singularity of these laws has not only caused them to be called in question, but even the existence of the conducting-power of gases to be contested. G. Wiedemann† supposes that in M. Becquerel's experiments the current may have been transmitted, not through the gas, but through the cements used for sealing the electrodes, they having become conductive in consequence of the rise of temperature: in support of his opinion he cites a negative experiment of Grove's‡.

In view of these divergences I have thought it would be useful to place beyond dispute the existence of the conductivity of gases, by means of an experiment in which every part of the apparatus would be constantly accessible to observation. For that purpose I had recourse to the following arrangement:—

Into a circuit the sulphate-of-copper cell and a capillary electrometer are introduced. The circuit is interrupted at one point; and each of the extremities of the wire is attached to a platinum disk of about 3 centims. diameter; the two disks are kept vertical, facing and parallel to one another, by means of long pipe-stems insulated at the lower part. The distance of the disks being regulated at 2 or 3 millims., it is clear that the circuit is interrupted by the interposed air, and that the electrometer remains motionless.

The experiment is commenced by closing the electrometer upon itself by means of a bridge annexed to it. Then, with the aid of an enameller's blow-pipe, the two platinum disks are made red-hot: during this operation the electrometer remains constantly at zero, since it is closed by the bridge. The flame is then removed, and, a moment after, the bridge; the mercury of the electrometer immediately goes out of the field of the microscope. Consequently the continuity of the circuit, which was interrupted by the cold air, is restored by the heated air, and no doubt can remain of the existence of the galvanic conductivity of hot gases.

M. Becquerel had verified the appearance of the conducting-power of gases only at red heat; I have been able to observe that power at much lower temperatures. The apparatus which I have employed is the same as the preceding, except that the pile consists of five Bunsens. An illuminating-gas flame placed below the

* *Annales de Chimie et de Physique*, sér. 3, t. xxxix. p. 355 (1853).

† Wiedemann, *Galv. u. Elektr.* Bd. i. p. 339.

‡ Athenæum, 1853, p. 1134; *Inst.* 1854, p. 35*.

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platinum disks, at a distance of 40 centims., is sufficient for demonstrating the passage of the electricity. The mean temperature of the gas is in this case so little elevated that a person can hold his hand in it; a thermometer placed there shows a mean final temperature of from 60° to 70°. The interposition of a screen or the agitation of the air hinders the phenomenon. The lamp can be replaced by an incandescent body, such as a stout glass rod made red-hot; consequently atmospheric air itself is susceptible of becoming a conductor. I have observed that if the two platinum disks are heated unequally, considerable electromotive forces are produced: the hotter disk constitutes a negative pole with respect to the other. The direction of this phenomenon is the same as that observed by M. Becquerel in the flame itself.—*Comptes Rendus de l'Académie des Sciences*, April 4, 1881, t. xcii. pp. 870–872.

ON AN ACOUSTIC PHENOMENON NOTICED IN A CROOKES TUBE.

BY CHARLES R. CROSS*.

A short time since, while experimenting with a Crookes tube, I noticed a phenomenon which was quite striking, and so evident that it hardly seems possible that it has not frequently been observed before; but as no allusion to the effect in question has come to my notice, I venture to call attention to it.

In working with the tube, in which a piece of sheet platinum is rendered incandescent by the concentration upon it of electrified particles repelled from a concave mirror, I noticed that when the mirror was made the negative electrode, so that this concentration took place, a clear and quite musical note issued from the tube. I thought at first that the pitch of the note would coincide with that produced by the circuit-breaker used with the coil (which made about 100 breaks per second); but this did not prove to be the case. In fact, very great changes in the rate of the circuit-breaker did not affect the note given by the tube. The effect seemed to be produced by the vibration of the sheet platinum in its own period, under the influence of the molecular impact, which vibration was communicated to the glass walls of the tube by the enamel rod to which the platinum was attached, giving rise to a sound somewhat resembling the pattering of rain against a window-pane, but higher in pitch and more musical. This sound changed its character very greatly when the direction of the current was reversed, a feeble murmur only being heard. I obtained a similar musical note, though far less loud, with the "mean free-path tube," best when the middle plate was positive. With a tube containing phosphorescent sulphide of calcium, the note was very dull in its quality and low in pitch, but still quite perceptible. With this tube a change in the direction of the current, as might be expected, did not affect the sound produced. I did not obtain this musical note from any tube that I have in which the current enters and leaves by a straight wire, except in the case of a single Geissler's tube exhausted so as to give stratifications, in which it was very feebly heard.—*American Journal of Otology*, January 1881.

* Read at a Meeting of the American Academy of Arts and Sciences, November 10, 1880.

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THE

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[FIFTH SERIES.]

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LV. *On the Molecular Vortex Theory of Electromagnetic Action.* By R. T. GLAZEBROOK, *M.A., Fellow of Trinity College, and Demonstrator of Experimental Physics at the Cavendish Laboratory, Cambridge**.

IN Crelle's *Journal*, vol. lxxii., Helmholtz ("Ueber die Bewegungsgleichungen der Electricität") called attention to the analogy between the equations of the electromagnetic field in a conductor and those which give the motion of a viscous fluid. The first part of the present paper is an attempt to develop more completely some of the consequences of that analogy.

Let us consider a medium in which ξ, η, ζ are the displacements at the time t, of the particle whose initial coordinates are x, y, z. Let ρ be the density of the medium, and $\omega_1, \omega_2, \omega_3$ the component angular velocities of the particle about the axes, and let $\dot{\xi} = \frac{d\xi}{dt}$, &c. Then we have

$$\left.\begin{aligned} \omega_1 &= \tfrac{1}{2}\left\{\frac{d\dot{\xi}}{dy} - \frac{d\dot{\eta}}{dz}\right\}, \\ \omega_2 &= \tfrac{1}{2}\left\{\frac{d\dot{\xi}}{dz} - \frac{d\dot{\zeta}}{dx}\right\}, \\ \omega_3 &= \tfrac{1}{2}\left\{\frac{d\dot{\eta}}{dx} - \frac{d\dot{\xi}}{dy}\right\}. \end{aligned}\right\} \quad \ldots \quad (1)$$

* Communicated by the Author.

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Let $$\nabla^2 = -\left\{\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right\},$$

$$\left.\begin{aligned}
\frac{d\omega_3}{dy}-\frac{d\omega_2}{dz} &= \tfrac{1}{2}\left\{\nabla^2\dot{\xi}+\frac{d}{dx}\left(\frac{d\dot{\xi}}{dx}+\frac{d\dot{\eta}}{dy}+\frac{d\dot{\zeta}}{dz}\right)\right\},\\
\frac{d\omega_1}{dz}-\frac{d\omega_3}{dy} &= \tfrac{1}{2}\left\{\nabla^2\dot{\eta}+\frac{d}{dy}\left(\frac{d\dot{\xi}}{dx}+\frac{d\dot{\eta}}{dy}+\frac{d\dot{\zeta}}{dz}\right)\right\},\\
\frac{d\omega_2}{dz}-\frac{d\omega_1}{dy} &= \tfrac{1}{2}\left\{\nabla^2\dot{\zeta}+\frac{d}{dz}\left(\frac{d\dot{\xi}}{dx}+\frac{d\dot{\eta}}{dy}+\frac{d\dot{\zeta}}{dz}\right)\right\}.
\end{aligned}\right\} \quad . \quad (2)$$

Now, according to the molecular vortex theory of magnetism, magnetic force is due to vortex motion in a fluid filling space, and the magnetic force at any point is equal to the molecular rotation of the vortex at that point. Let α, β, γ be the magnetic force at any point. Let F, G, H be the components of the vector potential or of the electrokinetic momentum; $\dot{f}, \dot{g}, \dot{h}$ the components of the electric current. Then we are to have on this hypothesis,

$$\alpha=\omega_1, \quad \beta=\omega_2, \quad \gamma=\omega_3.$$

But we know that, if μ is specific magnetic inductive capacity,

$$\left.\begin{aligned}
\mu\alpha &= \frac{dH}{dy}-\frac{dG}{dz}=\mu\omega_1,\\
\mu\beta &= \frac{dF}{dz}-\frac{dH}{dx}=\mu\omega_2,\\
\mu\gamma &= \frac{dG}{dx}-\frac{dF}{dy}=\mu\omega_3,
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (3)$$

Comparing these with (1), we have

$$\left.\begin{aligned}
F &= \tfrac{1}{2}\mu\dot{\xi},\\
G &= \tfrac{1}{2}\mu\dot{\eta},\\
H &= \tfrac{1}{2}\mu\dot{\zeta}.
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Again, we have from (2),

$$\nabla^2\dot{\xi}+\frac{d}{dx}\left(\frac{d\dot{\xi}}{dx}+\frac{d\dot{\eta}}{dy}+\frac{d\dot{\zeta}}{dz}\right)=2\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right)=8\pi\dot{f}, \quad (4\text{A})$$

since (Maxwell, 'Electricity and Magnetism,' p. 607, E)

$$\frac{d\gamma}{dy}-\frac{d\beta}{dz}=4\pi\dot{f}.$$

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Substitute for $\dot{\xi}, \dot{\eta}, \dot{\zeta}$ the values from (4), we get

$$4\pi\mu\dot{f} = \nabla^2 F + \frac{d}{dx}\left(\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz}\right), \quad \&c. \quad \&c.; \quad \ldots \quad (5)$$

and this agrees with Maxwell, p. 616 (4).

According to the molecular vortex theory (Phil. Mag. March 1861), μ is proportional to the density of the matter composing the vortices*, so that the momentum of an element parallel to the axis of x will be proportional to $\mu\dot{\xi}$ or F. Thus the momentum at any point in the direction S will be proportional to

$$F\frac{dx}{ds} + G\frac{dy}{ds} + H\frac{dz}{ds};$$

and this is what Maxwell calls the electrokinetic momentum.

Let us suppose our medium to be a viscous fluid, and let k be the coefficient of viscosity, ρ the density of the medium, X, Y, Z the forces per unit mass, p the pressure. Then we have

$$\rho\frac{d\dot{\xi}}{dt} = \rho X - \frac{dp}{dx} + \tfrac{1}{3}k\frac{d}{dx}\left(\frac{d\dot{\xi}}{dx} + \frac{d\dot{\eta}}{dy} + \frac{d\dot{\zeta}}{dz}\right) - k\nabla^2\dot{\xi}.$$

Substituting for $\dot{\xi}$ &c. and treating μ as constant,

$$\frac{2\rho}{\mu}\frac{dF}{dt} = \rho X - \frac{dp}{dx} + \tfrac{2}{3}\frac{k}{\mu}\frac{d}{dx}\left(\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz}\right) - \frac{2k}{\mu}\nabla^2 F.$$

Substitute for $\nabla^2 F$ from (5),

$$\frac{dF}{dt} = \frac{\mu}{2}\left(X - \frac{1}{\rho}\frac{dp}{dx}\right) - \frac{4\pi\mu k}{\rho}\dot{f} + \tfrac{4}{3}\frac{k}{\rho}\frac{d}{dx}\left(\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz}\right). \quad (6)$$

Now let us suppose that

$$\frac{\mu X}{2} = -\frac{dV}{dx},$$

and put

$$V + \frac{\mu p}{2\rho} - \tfrac{4}{3}\frac{k}{\rho}\left(\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz}\right) = \psi; \quad \ldots \quad (7)$$

then

$$\frac{dF}{dt} = -\frac{d\psi}{dx} - \frac{4\pi\mu k}{\rho}\dot{f}.$$

* If the vortices be circular and ρ the density, Maxwell shows that $\rho = \frac{\mu}{2}$, so that the electrokinetic momentum is equal to

$$F\frac{dx}{ds} + G\frac{dy}{ds} + H\frac{dz}{ds}.$$

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Let P be the electromotive force at the point parallel to axis of x, and let us call $\frac{4\pi\mu k}{\rho}$ the resistance of the medium; then Ohm's law gives us

$$P = \frac{4\pi\mu k}{\rho} \dot{f},$$

and we get

$$\left.\begin{aligned} P &= -\frac{dF}{dt} - \frac{d\psi}{dx}. \\ \text{Similarly} \\ Q &= -\frac{dG}{dt} - \frac{d\psi}{dy}, \\ R &= -\frac{dH}{dt} - \frac{d\psi}{dz}; \end{aligned}\right\} \quad \ldots\ldots \quad (8)$$

and these agree exactly with Maxwell (598, B), taking the case when the conductor is at rest.

Again, substituting in (6) for $\dot{f}$ its value from (5), we get, if

$$J = \frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz},$$

$$\frac{dF}{dt} + \frac{dV}{dx} + \frac{\mu}{2\rho}\frac{dp}{dx} + \frac{k}{\rho}\nabla^2 F - \tfrac{1}{3}\frac{k}{\rho}\frac{dJ}{dx} = 0. \quad (9)$$

Substituting and reducing,

$$\frac{dF}{dt} + \frac{d\psi}{dx} + \frac{1}{4\pi\mu C}\left(\nabla^2 F + \frac{dJ}{dx}\right) = 0, \quad \ldots \quad (10)$$

&c.,

C being the conductivity of the medium. And these, again, agree with Maxwell (783), supposing the medium we consider is a conductor, so that the quantity K in his expressions is equal to zero.

By differentiating we have

$$\frac{dJ}{dt} - \nabla^2\psi = 0.$$

From (8) $-\frac{d\psi}{dx}$ is the electromotive force at the point parallel to x, so far as it does not depend on electromagnetic action, arising, that is, from the action of the free electricity in the medium. And since we consider a conductor, we have

$$\nabla^2\psi = 0;$$

$$\therefore \frac{dJ}{dt} = 0.$$

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J = a constant = c say. But the equation of continuity gives us

$$J = -\frac{\mu}{2\rho}\frac{d\rho}{dt},$$

$$\rho = \rho_0 e^{-\frac{2ct}{\mu}}.$$

And this is impossible unless $c=0$, and $\rho=$ constant; in this case $J=0$. Thus we must suppose our fluid to be incompressible.

In forming equation (7), we have put

$$\frac{\mu X}{2} = -\frac{dV}{dx},$$

thus assuming that our forces at all points of the medium had a potential. If this be not so, let

$$\frac{\mu X}{2} = -P_1 - \frac{dV}{dx}.$$

Then the electromotive force P_1 acting at each point produces a current f_1, given by the equation

$$P_1 = \frac{4\pi\mu k}{\rho} f_1,$$

if f is the current arising from electromagnetic action,

$$f = f_1 + f',$$

and

$$P = P_1 + P',$$

where P' is the electromotive force arising from electromagnetic action. Thus

$$P' = -\frac{dF}{dt} - \frac{d\psi}{dx},$$

&c.

Again, since $J=0$,

$$\psi = V + \frac{\mu p}{2\rho}.$$

ψ is the potential of the free electricity in the medium; if the fluid be in equilibrium, the equations give us

$$\frac{d\psi}{dx}=0, \quad \frac{d\psi}{dy}=0, \quad \frac{d\psi}{dz}=0.$$

Also

$$F=G=H=0.$$

Thus, within a conductor in which the fluid is at rest there is no electromotive force at any point. Thus the electrical phenomena that occur in a conducting medium subject to elec-

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tromagnetic action may be explained by the hypothesis that the conducting medium is filled with a viscous fluid of constant density; forces X, Y, Z being applied to this medium at any point, rotational motion is set up.

The electromotive force parallel to the axis of x at any point, so far as it depends on external electrification, is

$$\frac{\mu}{2}\left(\mathrm{X}-\frac{1}{\rho}\frac{dp}{dx}\right)^*,$$

p being the pressure in the fluid, and μ the magnetic specific inductive capacity. The magnetic force in the same direction is ω_1, the angular velocity of the fluid about the axis of x. The electromotive force arising from electromagnetic action is

$$-\frac{\mu}{2}\frac{d^2\xi^*}{dt^2},$$

ξ being the displacement of a particle of the fluid. By the action of these electromotive forces currents may be produced at any point, and the current at a point parallel to $0x$ is

$$\frac{1}{8\pi}\nabla^2\left(\frac{d\xi}{dt}\right).$$

If k is the viscosity of the medium and C its electrical conductivity, then

$$\frac{1}{\mathrm{C}}=\frac{4\pi\mu k}{\rho}^*.$$

Now let C become very small; then k becomes very large, the medium ceases to be a viscous fluid and becomes an elastic solid. Equations (1), (2), (3), (4), (5) hold; but instead of (6) we have

$$\rho\frac{d\dot{\xi}}{dt}=\rho\mathrm{X}+\mathrm{A}\frac{d}{dx}\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right)-\mathrm{B}\nabla^2\xi. \quad . \quad . \quad (11)$$

But, integrating (4A),

$$\nabla^2\xi+\frac{d}{dx}\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right)=8\pi f. \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Assuming that f is zero everywhere initially,

$$\therefore \frac{d\mathrm{F}}{dt}=\frac{\mu}{2}\left\{\mathrm{X}+\frac{\mathrm{A}+\mathrm{B}}{\rho}\frac{d}{dx}\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right)\right\}-\frac{4\pi\mu}{\rho}\mathrm{B}f. \quad (13)$$

Let

$$\frac{\mu}{2}\left\{\mathrm{X}+\frac{\mathrm{A}+\mathrm{B}}{\rho}\frac{d}{dx}\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right)\right\}=-\frac{d\psi}{dx},$$

* All these expressions are of course simplified if, with Maxwell, we put $\frac{\mu}{2}=\rho$.

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let P be the electromotive force at the point, and suppose that

$$\frac{\rho}{\mu B} = K,$$

the specific inductive capacity; then

$$\left.\begin{aligned} P &= \frac{4\pi}{K} f = \frac{4\pi\mu B}{\rho} f; \\ \therefore P &= -\frac{dF}{dt} - \frac{d\psi}{dx}. \end{aligned}\right\}$$

Similarly

$$\left.\begin{aligned} Q &= -\frac{dG}{dt} - \frac{d\psi}{dy}, \\ R &= -\frac{dH}{dt} - \frac{d\psi}{dz}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (14)$$

And thus in a dielectric medium also Maxwell's equations would hold.

Differentiating (12) and the two similar equations with reference to x, y, z and adding, we have

$$\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz} = 0. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Differentiating (14) with reference to t, and substituting for P from (12), remembering that

$$\frac{\mu\dot{\xi}}{2} = F \text{ \&c.},$$

we have, of course, Maxwell's equations for F, G, H in a dielectric medium, viz.

$$\mu K \frac{d}{dt}\left(\frac{dF}{dt} + \frac{d\psi}{dx}\right) + \nabla^2\psi + \frac{dJ}{dx} = 0, \text{ \&c.} \quad . \quad (16)$$

Maxwell, § 783 (7). If J is a linear function of t, or a constant, or zero,

$$\frac{d}{dx}\left(\frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz}\right) = 0,$$

and

$$-\frac{d\psi}{dx} = \frac{\mu X}{2}.$$

Thus the electromotive force at any point due to the free electricity is proportional to the mechanical force exerted at that point in the medium. In addition to this we have the

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electromotive force $-\frac{dF}{dt}$, which arises from the electromagnetic action. Thus, in a dielectric, electromagnetic phenomena may be explained by the strains in an elastic solid. If K be the specific inductive capacity of the dielectric medium, and B the coefficient of rigidity of the elastic solid,

$$\frac{1}{K}=\frac{\mu B}{\rho}.$$

If we suppose the motion such that

$$\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dt}=0,$$

so that the solid is incompressible, the electric displacement at any point in the direction of x is given by $\frac{1}{8\pi}\nabla^2\xi$. The magnetic force is equal to the molecular rotation at that point in the solid.

In the paper in the Philosophical Magazine already referred to, Prof. Maxwell has shown that the state of stress which exists in the magnetic field is just that which would be produced by vortices in an incompressible fluid. To account for electricity, he supposes that there are a number of moving particles between these vortices. If we suppose that in a conductor Maxwell's moving medium behaves like a viscous fluid, while in a dielectric its properties are those of an elastic solid, the electrical current at any point is the "concentration" of the velocity at that point, and the electrical displacement the "concentration" of the displacement; and the additional "idle wheels" become unnecessary, except as explaining how the vortices may be conceived to rotate.

So far we have been considering the analogy between the motion of the elastic solid and electromagnetic action. Let us make the assumption that magnetic force in a dielectric arises from molecular vortices in a medium which may be treated like an elastic solid, and let us suppose that, owing to a wave of displacement travelling through this medium, the vortices are displaced, and a term arises in the kinetic energy of the form

$$2\overline{C}(\alpha\omega_1+\beta\omega_2+\gamma\omega_3),$$

ω_1, ω_2, ω_3 being the angular velocities of the element considered which arise from the displacement ξ, η, ζ. Then Maxwell has shown that the kinetic energy T per unit volume is given by

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$$T=\tfrac{1}{2}\rho(\dot{\xi}^2+\dot{\eta}^2+\dot{\zeta}^2)+\bar{C}\Big\{\dot{\xi}\frac{d}{d\theta}\Big(\frac{d\xi}{dy}-\frac{d\eta}{dz}\Big)+\dot{\eta}\frac{d}{d\theta}\Big(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\Big)+\dot{\zeta}\frac{d}{d\theta}\Big(\frac{d\eta}{dx}-\frac{d\xi}{dy}\Big)\Big\}, \quad (17)$$

where

$$\frac{d}{d\theta}=\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz};$$

and from this he has obtained an expression for the rotation of the plane of polarization of light in the field.

I wish to put this in a somewhat more general form, in order to develop the connexion between it and the electromotive force discovered experimentally by Mr. E. H. Hall (Phil. Mag. March 1880), which leads, as Prof. Rowland has shown, to the same expression for the rotation of the plane of polarization of the light. Let us suppose, then, that forces X, Y, Z are applied per unit mass at each point of our medium, and also that we may consider it as incompressible. Then, if W be the potential energy and B the coefficient of rigidity,

$$-W=\rho(X\delta\xi+Y\delta\eta+Z\delta\zeta)+\frac{B}{2}\Big\{\Big(\frac{d\xi}{dy}+\frac{d\eta}{dx}\Big)^2+\Big(\frac{d\xi}{dz}+\frac{d\zeta}{dx}\Big)^2+\Big(\frac{d\eta}{dz}+\frac{d\zeta}{dy}\Big)^2-4\Big(\frac{d\eta}{dy}\frac{d\zeta}{dz}+\frac{d\xi}{dx}\frac{d\zeta}{dz}+\frac{d\xi}{dx}\frac{d\eta}{dy}\Big)\Big\}. \quad (18)$$

To obtain the equations of motion we follow the method adopted by Fitzgerald ("On the Electromagnetic Theory of the Reflexion and Refraction of Light," Phil. Trans. 1880); and from the condition that

$$\delta\int(T+W)dt=0,$$

we arrive finally at the equations

$$\rho\ddot{\xi}+2\bar{C}\frac{d}{d\theta}\Big(\frac{d\dot{\zeta}}{dy}-\frac{d\dot{\eta}}{dz}\Big)=\rho X-B\nabla^2\xi \text{ \&c.} \quad (19)$$

Now we have seen already that, on the molecular vortex theory, if α', β', γ' denote the magnetic force, f, g, h the electric displacement, and F, G, H the components of vector potential due to the displacements considered,

$$\alpha'=\omega_1=\tfrac{1}{2}\Big(\frac{d\dot{\zeta}}{dy}-\frac{d\dot{\eta}}{dz}\Big) \text{ \&c.},$$

$$f=\frac{1}{8\pi}\cdot\nabla^2\xi \text{ \&c.},$$

$$F=\frac{\mu\dot{\xi}}{2} \text{ \&c.}$$

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Thus, substituting for ξ &c.,

$$\frac{dF}{dt} + \frac{2\mu\bar{C}}{\rho}\frac{d\alpha'}{d\theta} = \frac{\mu X}{2} - \frac{4\pi B\mu}{\rho} f.$$

Now
$$\frac{d\alpha'}{d\theta} = \alpha\frac{d\alpha'}{dx} + \beta\frac{d\alpha'}{dy} + \gamma\frac{d\alpha'}{dz}$$

$$= \alpha\frac{d\alpha'}{dx} + \beta\frac{d\beta'}{dx} + \gamma\frac{d\gamma'}{dx} + 4\pi(\gamma\dot{g} - \beta\dot{h})$$

$$= \frac{d}{dx}(\alpha\alpha' + \beta\beta' + \gamma\gamma') + 4\pi(\gamma\dot{g} - \beta\dot{h}),$$

since α, β, γ are constant. Put $-\frac{d\psi}{dx}$ for $\frac{\mu X}{2}$, the total electromotive force impressed on the element parallel to the axis of x, and, as before, let

$$\frac{\mu B}{\rho} = \frac{1}{K},$$

K being the specific inductive capacity; then, if P be the electromotive force parallel to x,

$$P = \frac{4\pi f}{K}.$$

Then

$$\frac{dF}{dt} + \frac{8\pi\mu\bar{C}}{\rho}(\gamma\dot{g} - \beta\dot{h}) + \frac{2\mu\bar{C}}{\rho}\frac{d}{dx}(\alpha\alpha' + \beta\beta' + \gamma\gamma') + P = -\frac{d\psi}{dx}. \quad . \quad . \quad . \quad (20)$$

. . . . . .

Thus P, the electromotive force parallel to x, is, on the molecular vortex theory, composed of four parts in a field of strong magnetic force. The first is $-\frac{dF}{dt}$, the rate of change of the vector potential. The next is

$$-\frac{8\pi\mu\bar{C}}{\rho}(\gamma\dot{g} - \beta\dot{h});$$

and this is exactly similar in form to the electromotive force discovered by Mr. E. H. Hall and already alluded to. The third may be written

$$-\frac{2\mu\bar{C}}{\rho}\frac{d}{dx}(\mathfrak{h}\,\mathfrak{h}'\cos\epsilon);$$

where $\mathfrak{h}$ is the impressed magnetic force, $\mathfrak{h}'$ the resultant of the force arising from the displacement of the medium, and ϵ the angle between them. In Mr. Hall's experiments this angle would be 90°, and therefore the term would vanish.

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The fourth term $-\frac{d\psi}{dx}$ is, of course, that part of the electromotive force which arises from the electricity in the field. Had we considered the case of a conductor, the terms which we should have to add to equations (8) would be just the same as those added above in (20) to equation (14). Thus, in a conductor we should have

$$\frac{d\mathrm{F}}{dt}+\frac{8\pi\mu\overline{\mathrm{C}}}{\rho}(\gamma\dot{g}-\beta\dot{h})+\frac{2\mu\overline{\mathrm{C}}}{\rho}\frac{d}{dx}(\alpha\alpha'+\beta\beta'+\gamma\gamma')$$

$$\&\mathrm{c.}\qquad +\mathrm{P}=-\frac{d\psi}{dx}\quad\ldots\quad(21)$$

In the case considered by Mr. Hall,

$$\frac{d\mathrm{F}}{dt}=0,\quad \alpha\alpha'+\beta\beta'+\gamma\gamma'=0,$$

and

$-\frac{d\psi}{dx}=$ impressed electromotive force in direction of $x=\mathrm{P}_1$ say;

$$\therefore\ \mathrm{P}=\mathrm{P}_1-\frac{8\pi\mu\overline{\mathrm{C}}}{\rho}(\gamma\dot{g}-\beta\dot{h}).\quad\ldots\quad(22)$$

Thus, if a current $\dot{f}$ flow in a conductor in a field of strong magnetic force γ, there will be produced an electromotive force in the direction of y (perpendicular, that is, to the directions of $\dot{f}$ and γ), whose value will be

$$+\frac{8\pi\mu\overline{\mathrm{C}}}{\rho}\gamma\dot{f}.$$

If we consider a wave travelling through the medium, the electromotive force parallel to x produced by electromagnetic action will be

$$-\frac{d\mathrm{F}}{dt}-\frac{8\pi\mu\overline{\mathrm{C}}}{\rho}(\gamma\dot{g}-\beta\dot{h}).$$

Substituting in the equations

$$-\mu\mathrm{K}\frac{d\mathrm{P}}{dt}+\nabla^2\mathrm{F}=0,$$

we get

$$\mu\mathrm{K}\left\{\frac{d^2\mathrm{F}}{dt^2}+\frac{8\pi\mu\overline{\mathrm{C}}}{\rho}\frac{d}{dt}(\gamma\dot{g}-\beta\dot{h})\right\}+\nabla^2\mathrm{F}=0;$$

and from these Professor Rowland has calculated the magnetic rotation of the vectors F, G, H (Phil. Mag. April 1881).

The equations satisfied by the magnetic force α', β', γ', can be found either from these by differentiation, or from the original equations (19). We get, remembering that

$$\dot{f}=\frac{1}{4\pi}\left(\frac{d\gamma'}{dy}-\frac{d\beta'}{dz}\right),$$

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$$\mu K\left\{\frac{d^2\alpha'}{dt^2}+\frac{2\bar{C}}{\rho}\frac{d}{dt}\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\left(\frac{d\gamma'}{dy}-\frac{d\beta'}{dz}\right)\right\}$$

$$\&c. \qquad +\nabla^2\alpha'=0, \quad . \quad (23)$$

These or the original equations (19), if we put $\frac{B}{\rho}=\frac{1}{\mu K}$, and X the impressed electromotive force $=0$, and substitute for $\frac{2\mu\bar{C}}{\rho}$, $8\pi C$, agree with Mr. Fitzgerald's equation. If we consider that the magnetic force is parallel to z and that our wave is travelling in that direction, Mr. Fitzgerald has shown that the wave must consist of two circularly polarized waves travelling respectively with velocities v_1 and v_2, where

$$v_1=\frac{1}{\sqrt{\mu K}}+\frac{2\pi\bar{C}\gamma}{\rho\lambda},$$

and

$$v_2=\frac{1}{\sqrt{\mu K}}-\frac{2\pi\bar{C}\gamma}{\rho\lambda}.$$

So that the rotation of the plane of polarization produced by a length l of the substance is

$$\frac{4\pi\bar{C}M\iota^2}{V_0\rho\lambda_0^2}\left(\iota-\lambda\frac{d\iota}{d\lambda}\right)l,$$

ι being the refractive index, V_0 and λ_0 the velocity and wave-length in air, and M the magnetic force. The rotation of unit length for unit magnetic force will be $\frac{4\pi C\iota^3}{V_0\rho\lambda_0^2}$ if we neglect $\frac{d\iota}{d\lambda}$. According to Mr. J. E. H. Gordon (Phil. Trans. 1877), the value of this quantity for the thallium-ray in carbon disulphide is $3{\cdot}047\times10^{-5}$ about. Also

$$\lambda_0=5{\cdot}349\times10^{-5},$$

$$V_0=3\times10^{10},$$

$$V_0\lambda_0^2=75\ ;$$

ρ is the density of the medium—of course not the dielectric, but the æther. According to Maxwell ("Physical Lines of Force," Phil. Mag. April 1861), this is of the same order as μ, being equal to $\frac{\mu}{2}$ if the vortices are circular, and μ is nearly unity. Thus

$$\bar{C}=\frac{3{\cdot}047\times10^{-5}\times75}{4\pi\iota^3};$$

and $\bar{C}$ is of order

$$10^{-5} \text{ or } 10^{-6}.$$

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In his paper Prof. Rowland remarks, after showing that Mr. Hall's discovery leads to the same expressions for the rotation of the plane as Prof. Maxwell's, "the conclusion we draw is that *the effect discovered by Mr. Hall is the same or due to the same cause as the rotation of the plane of polarization of light.*" The previous investigation, I think, may supply a connecting link between the two.

As has already been stated, equations (19) and (23) agree in form with Mr. Fitzgerald's equations; and in fact (23) is identical with his. For let us put

$$\dot{p}=\alpha', \quad \dot{q}=\beta', \quad \dot{r}=\gamma';$$

then p, q, r are the same as Mr. Fitzgerald's ξ, η, ζ, and his final equation is

$$\mu k\left\{\frac{d^2p}{dt^2}+8\pi C\frac{d}{dt}\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\left(\frac{dr}{dy}-\frac{dq}{dr}\right)\right\} \qquad \&c.; \qquad +\nabla^2 p=0, \quad . \quad . \quad (24)$$

and differentiating these with respect to t and substituting, we get (23).

Now Mr. Fitzgerald's equation has been deduced, without any assumption as to the connexion between the motions of matter and æther, from the hypothesis that the electrokinetic energy of the medium contains a term

$$4\pi C\iiint\left\{\frac{dp}{d\theta}\dot{f}+\frac{dq}{d\theta}\dot{g}+\frac{dr}{d\theta}\dot{h}\right\}dx\,dy\,dz,$$

where

$$\frac{d}{d\theta}=\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}.$$

Professor Maxwell's additional term depends on the displacements of the medium which constitute light. The potential energy in his case is that which arises from the state of strain of the medium, while in Mr. Fitzgerald's paper the potential energy is the electrostatic energy of the field. The foregoing investigation shows that the two assumptions lead to the same equations for the electric and magnetic disturbance.

If we suppose that we have in the field light-motion in addition to electrical and magnetic displacements, and assume with Prof. Maxwell his additional term in the kinetic energy, then, on the molecular vortex theory, the electrokinetic energy, or that part of the kinetic energy on which electrical and magnetic effects depend, will contain a term of the form supposed by Mr. Fitzgerald. The kinetic energy being

$$\frac{\rho}{2}\iiint(\dot{\xi}^2+\dot{\eta}^2+\dot{\zeta}^2)\,dx\,dy\,dz,$$

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the electrokinetic is

$$\frac{\mu}{8\pi}\iiint(\dot{p}^2+\dot{q}^2+\dot{r}^2)\,dx\,dy\,dz,$$

being obtained from the kinetic by substituting $\frac{\mu}{4\pi}$ for ρ, and p, q, r for ξ, η, ζ. If, then, in the kinetic energy we have the term

$$\bar{C}\left\{\dot{\xi}\frac{d}{d\theta}\left(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\right)+\text{ \&c.}\right\}dx\,dy\,dz,$$

in the electrokinetic energy we have

$$\bar{C}\left\{\dot{p}\frac{d}{d\theta}\left(\frac{dr}{dy}-\frac{dq}{dz}\right)+\text{ \&c.}\right\}dx\,dy\,dz;$$

and this, on integrating by parts, gives

$$4\pi\bar{C}\left\{\dot{f}\frac{dp}{d\theta}+\dot{g}\frac{dq}{d\theta}+\dot{h}\frac{dr}{d\theta}\right\}dx\,dy\,dz,$$

which is Mr. Fitzgerald's additional term.

In fact, if we consider the medium as consisting of a number of separate molecules, the kinetic energy has its ordinary meaning; the electrokinetic is that part of the kinetic energy which depends only on the rotation of the molecules; for this rotation alone produces magnetic force. If a transverse wave of disturbance travels through the medium, the translational motion of each molecule becomes known to us as light, while to the rotational velocity we give the name of magnetic force. Since the direction of rotation at each point is reversed many times a second, we cannot produce magnetic force by a wave of light. If, however, magnetic force exists in the medium independently of the light, the translational motion of the molecules is modified thereby.

Now let us consider a molecule of the medium, which we shall suppose moves as a rigid body, with angular velocities $\omega_1, \omega_2, \omega_3$. If ξ, η, ζ are the coordinates of the centre of gravity, and $dx\,dy\,dz$ the volume, A, B, C the radii of gyration about axes parallel to those of coordinates, which we assume to be principal axes, then the kinetic energy of this molecule is

$$\tfrac{1}{2}\rho(\dot{\xi}^2+\dot{\eta}^2+\dot{\zeta}^2)\,dx\,dy\,dz+\rho(A^2\omega_1^2+B^2\omega_2^2+C^2\omega_3^2)\,dx\,dy\,dz.$$

In a material medium, A, B, C being proportional to the linear dimensions of the molecule, the last term, $A^2\omega_1^2+B^2\omega_2^2+C^2\omega_3^2$, vanishes compared with the other, the molecule being very

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small. Let us suppose that in the æther, $\omega_1, \omega_2, \omega_3$ become known to us as magnetic force; then this last term expresses the magnetic energy in the field. And if we suppose, further, that

$$A^2 = B^2 = C^2 = \frac{\mu}{8\pi\rho},$$

since $\omega_1 = \alpha$ &c., the magnetic energy of the whole field may be written

$$\frac{\mu}{8\pi}\iiint(\alpha^2+\beta^2+\gamma^2)\,dx\,dy\,dz;$$

and this may be transformed to

$$\frac{\mu}{16\pi}\iiint\{\dot{\xi}\nabla^2\dot{\xi} + \dot{\eta}\nabla^2\dot{\eta} + \dot{\zeta}\nabla^2\dot{\zeta}\}\,dx\,dy\,dz$$

by an application of Green's theorem whenever ξ, η, ζ are functions of the same function of x, y, z. Hence, remembering that

$$\nabla^2\dot{\zeta} = 8\pi\dot{f}, \quad \frac{\mu\dot{\zeta}}{2} = F, \dot{f} = u \text{ &c.},$$

the electrokinetic energy becomes

$$\iiint(Fu + Gv + Hw)\,dx\,dy\,dz.$$

Since in the kinetic energy of the field we have, on the molecular vortex theory, a term involving $(\omega_1^2+\omega_2^2+\omega_3^2)$, we see that if we put for ω_1

$$\alpha + \omega_1,$$

α being the rotation which constitutes the magnetic force, and ω_1 the rotation due to the wave of displacement travelling through, we shall introduce a term $\alpha\omega_1 + \beta\omega_2 + \gamma\omega_3$, which is the hypothesis from which we started.

If we start from Mr. Fitzgerald's standpoint, and assume terms in the electrokinetic energy of the form

$$4\pi C\iiint\left(\dot{f}\,\frac{dp}{d\theta} + \dot{g}\,\frac{dq}{d\theta} + \dot{h}\,\frac{dr}{d\theta}\right)dx\,dy\,dz,$$

to explain the phenomena of the rotation of the plane of polarization without reference to theories of molecular vortices, we may show that this assumption also leads to Hall's effect. We wish to transform this term into one in which the kinetic energy is expressed in terms of $f, g, h, \dot{f}, \dot{g}, \dot{h}$ and constants. Then, if T be the kinetic energy, and P, Q, R the components

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of the electromotive force,

$$\left.\begin{aligned} -\mathrm{P} &= \frac{d}{dt}\frac{d\mathrm{T}}{d\dot{f}} - \frac{d\mathrm{T}}{df}, \\ -\mathrm{Q} &= \frac{d}{dt}\frac{d\mathrm{T}}{d\dot{g}} - \frac{d\mathrm{T}}{dg}, \\ -\mathrm{R} &= \frac{d}{dt}\frac{d\mathrm{T}}{d\dot{h}} - \frac{d\mathrm{T}}{dh}. \end{aligned}\right\} \quad \ldots\ldots \quad (25)$$

Now

$$\iiint \dot{f}\frac{dp}{d\theta}\,dx\,dy\,dz = \iiint \dot{f}\left(\alpha\frac{dp}{dx} + \beta\frac{dp}{dy} + \gamma\frac{dp}{dz}\right)dx\,dy\,dz.$$

But we have, from the definitions of p, q, r,

$$\frac{dp}{dy} = \frac{dq}{dx} - 4\pi h,$$

$$\frac{dp}{dz} = \frac{dr}{dx} + 4\pi g.$$

Thus the term in $\dot{f}$ becomes

$$4\pi\iiint \dot{f}(g\gamma - h\beta)\,dx\,dy\,dz + \iiint \dot{f}\left(\alpha\frac{dp}{dx} + \beta\frac{dq}{dx} + \gamma\frac{dr}{dx}\right)dx\,dy\,dz.$$

Integrate the last term by parts; then, if we neglect the surface-integral, we have

$$-\iiint\left[p\frac{d}{dx}(\alpha\dot{f}) + q\frac{d}{dx}(\beta\dot{f}) + r\frac{d}{dx}(\gamma\dot{f})\right]dx\,dy\,dz,$$

and the additional term in the kinetic energy becomes

$$16\pi^2\mathrm{C}\iiint[\dot{f}(\gamma g - \beta h) + \dot{g}(\alpha h - \gamma f) + \dot{h}(\beta f - \alpha g)]\,dx\,dy\,dz$$

$$-4\pi\mathrm{C}\iiint\left[p\left\{\frac{d}{dx}(\alpha\dot{f}) + \frac{d}{dy}(\alpha\dot{g}) + \frac{d}{ds}(\alpha\dot{h})\right\}\right.$$

$$+q\left\{\frac{d}{dx}(\beta\dot{f}) + \frac{d}{dy}(\beta\dot{g}) + \frac{d}{dz}(\beta\dot{h})\right\}$$

$$\left.+r\left\{\frac{d}{dx}(\gamma\dot{f}) + \frac{d}{dy}(\gamma\dot{g}) + \frac{d}{dz}(\gamma\dot{h})\right\}\right]dx\,dy\,dz.$$

The coefficient of p in the last, on substituting for $\dot{f}, \dot{g}, \dot{h}$ the values

$$\frac{1}{4\pi}\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right) \&c.$$

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and reducing, becomes

$$\frac{d\alpha}{dx}\frac{d\gamma}{dy}-\frac{d\alpha}{dy}\frac{d\gamma}{dx}+\frac{d\alpha}{dz}\frac{d\beta}{dx}-\frac{d\alpha}{dx}\frac{d\beta}{dz}.$$

And this expression vanishes whenever α, β, γ are functions of the same function of x, y, and z. This is, of course, satisfied if α, β, γ are constants or components of a wave of magnetic force crossing the medium.

Hence, whenever a wave is traversing a field of magnetic force, the electrokinetic energy is

$$\iiint [\mathrm{F}\dot{f}+\mathrm{G}\dot{g}+\mathrm{H}\dot{h}+16\pi^2\mathrm{C}\{\dot{f}(\gamma g-\beta h)+\dot{g}(\alpha h-\gamma f)+\dot{h}(\beta f-\alpha g)\}]\,dx\,dy\,dz. \quad . \quad (26)$$

The terms in C are in general small; and if, with Mr. Fitzgerald, we are considering a wave in a field of strong uniform magnetic force components $\bar{\alpha}, \bar{\beta}, \bar{\gamma}$, we may put $\bar{\alpha}, \bar{\beta}, \bar{\gamma}$ for α, β, γ in the above terms and treat them as constant. Then, from the equations

$$\mathrm{P}=-\frac{d}{dt}\frac{d\mathrm{T}}{d\dot{f}}-\frac{d\mathrm{T}}{df}$$

we get

$$\left.\begin{aligned}\mathrm{P}&=-\frac{d\mathrm{F}}{dt}-32\pi^2\mathrm{C}(\gamma\dot{g}-\beta\dot{h}),\\ \mathrm{Q}&=-\frac{d\mathrm{G}}{dt}-32\pi^2\mathrm{C}(\alpha\dot{h}-\gamma\dot{f}),\\ \mathrm{R}&=-\frac{d\mathrm{H}}{dt}-32\pi^2\mathrm{C}(\beta\dot{f}-\alpha\dot{g}).\end{aligned}\right\} \quad . \quad . \quad (27)$$

And these agree exactly with (21), remembering that

$$\frac{\mu\bar{\mathrm{C}}}{\rho}=4\pi\mathrm{C},$$

and that in the present case we have neglected quantities like α', β', γ'. Thus, the additional term assumed by Mr. Fitzgerald leads to Mr. Hall's additional terms in the electromotive force. Of course, if we start from Mr. Hall's terms in the electromotive force, and work backwards to find the electrokinetic energy, we shall arrive at Mr. Fitzgerald's term; and if, further, we assume the hypotheses of the molecular vortex theory, we shall get Maxwell's additional term. Mr. Fitzgerald's term is a direct consequence of Hall's experiments; Maxwell's term is a consequence of them on some theory of the action between light and magnetism.

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LVI. *Electric Absorption of Crystals.* *By* H. A. Rowland *and* E. H. Nichols, *of the Johns Hopkins University, Baltimore* *.

[Plate IX.]

I.

THE theory of electric absorption does not seem to have as yet attracted the general attention which its importance demands; and from the writings of many physicists we should gather the impression that the subject is not thoroughly understood. Nevertheless the subject has been reduced to mathematics; and a more or less complete theory of it has been in existence for many years. Clausius seems to have been the first to give what is now considered the best theory. His memoir, "On the Mechanical Equivalent of an Electric Discharge," &c., was read at the Berlin Academy in 1852 †. In an addition to this memoir in 1866 he shows that a dielectric medium having in its mass particles imperfectly conducting would have the property of electric absorption. Maxwell, in his 'Electricity,' art. 325, gives this theory in a somewhat different form, and shows that a body composed of layers of different substances would possess the property in question. One of us, in a note in the 'American Journal of Mathematics,' No. 1, 1878, put the matter in a somewhat different form, and investigated the conditions for there being no electric absorption.

All these theories agree in showing that there should be no electric absorption in a perfectly homogeneous medium. A mass of glass can hardly be regarded as homogeneous, seeing that when we keep it melted for a long time a portion separates out in crystals. Glass can thus be roughly regarded as a mass of crystals with their axes in different directions in a medium of a different nature. It should thus have electric absorption. Among all solid bodies, we can select none which we can regard as perfectly homogeneous along any given line through them, except crystals. The theory would then indicate that crystals should have no electric absorption; and it is the object of this paper to test this point. The theory of both Clausius and Maxwell refers only to the case of a condenser made of two parallel planes. In the

* Communicated by the Physical Society, having been read May 14th, 1881.

† I have obtained my knowledge of this memoir from the French translation, entitled *Théorie Mécanique de la Chaleur*, par R. Clausius, translated into French by F. Folie: Paris, 1869. The "Addition" does not appear in the memoir published in Pogg. *Ann.* vol. lxxxvi. p. 337, but was added in 1866 to the collection of memoirs.

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"Note" referred to, one of us has shown that in other forms of condenser there can be electric absorption even in the case of homogeneous bodies. Hence the problem was to test the electric absorption of a crystal, in the case of an infinite plate of crystal with parallel sides. The considerations with regard to the infinite plate were avoided by using the guard-ring principle of Thomson.

The crystals which could be obtained in large and perfect plates were quartz and calcite. These were of a rather irregular form, about 35 millim. across and $1\frac{1}{2}$ millim. thick, and perfectly ground to plane parallel faces. There were two quartz plates cut from the same crystal perpendicular to the axis, and two cleavage-plates of Iceland spar. There were also several specimens of glass ground to the same thickness; the plates were all perfectly transparent, with polished faces. Examined by polarized light, the quartz plates seemed perfectly homogeneous at all points except near the edge of one of them. This one showed traces of amethystine structure at that point; and a portion of one edge had a piece of quartz of opposite rotation set in; but the portion which was used in the experiment was apparently perfectly regular in structure. The fact that there are two species of quartz, right- and left-handed, with only a slight change in their crystalline structure, and that, as in amethyst, they often occur together, makes it not improbable that most pieces of right-handed quartz contain some molecules of left-handed quartz, and *vice versâ*. In this case quartz might possess the property of electric absorption to some degree. But Iceland spar should evidently more nearly satisfy the conditions. It is unfortunate that the two pieces of quartz were not cut from different crystals.

This reasoning was confirmed by the experiments, which showed that the quartz had about one ninth the absorption of glass; but that the Iceland spar had *none* whatever, and is thus the first solid so far found having no electric absorption. Some crystals of mica &c. were tried; but calc spar is the only one which we can say, *à priori*, is perfectly homogeneous. Thus mica and selenite are so very lamellar in their character, that few specimens ever appear in which the laminæ are not more or less separated from one another; and thus they should have electric absorption.

II.

In the ordinary method of experimenting with the various forms of Leyden jar, there are, besides the residual discharge due to electric absorption in the substance of the insulator,

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two other sources of a return charge. The surface of the glass being more or less conducting, an electric charge creeps over the surface from the edges of the tinfoil. In discharging the jar in the usual way by a connecting wire, this surface remains charged, and the electricity is gradually conducted back to the coatings, and thus recharges them. If, furthermore, the coatings be fastened to the glass with shellac or other cement, the return charge may be partly due to it; for we have between the coatings not merely glass, but layers of glass, cement, &c., which the theory shows to give a residual discharge. Besides the coatings are not planes; and hence, as one of us has shown, there may be a return charge, even if the glass gave none between infinite planes. If the plates were merely laid on the glass without cementing, the same result would follow, since the insulator would then consist of air and glass in layers.

In the present research these were sources of error to be avoided, since the residual discharge due to the insulating plates themselves were to be compared. The condenser-plates were copper disks. These were amalgamated, so that there was a layer of mercury between them and the dielectric, which excluded the air and conducted the electricity directly to the surface of the dielectric: thus the condition of a single substance between the plates was fulfilled. The errors due to the creeping of the charge over the surface of the dielectric and that due to the plates not being infinite were avoided, the first entirely and the second partially, by the use of the guard-ring principle of Sir Wm. Thomson.

Fig. 1 (Pl. IX.) represents this apparatus. The plate of crystal, *c*, was placed between two amalgamated plates of copper, *a* and *b*, over the upper one of which the guard-ring, *d*, was carefully fitted; this ring, when down, served to charge and discharge the surface around the plate, *a*; and so the errors above referred to from the creeping of the charge along the plate, and from the plate not being infinite, were avoided.

The charging battery consisted of six large Leyden jars of nearly a square foot of coated surface each, charged to a small potential. Although accurate instruments were at hand for measuring the potential in absolute measure, it was considered sufficient to use a Harris unit-jar for giving a definite charge; for very accurate measurements were not desired, and the Harris unit-jar was entirely sufficient for the purpose. The return charge was measured by a Thomson quadrant-electrometer of the original well-known form.

The apparatus shown in fig. 1 performs all the necessary operations by a half turn of the handle *e*. By two half turns

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of the handle, one forward and the other back, the crystal condenser could be successively charged from the Leyden battery, discharged, the guard-ring raised, the upper plate, *a*, again insulated, and the connexion made with the quadrant-electrometer.

The copper ring, *d*, was suspended by three silk threads from the brass disk, *f*, which in turn could be raised and lowered by the crank, *g*. A small wire connected the ring with the rod on which was the ball *h*. This rod was insulated by the glass tube *i*, and could revolve about an axis at *k*. By the up-and-down motion of the rod the ball came into contact with the ball (*l*) connected with the earth, or the ball (*m*) connected with the battery. When the cranks were in the position shown in the figure, the heavy ball *n* caused the ball *h* to rise and press against *l*; but when *f* descended, the piece *o* pressed on the rod and caused *h* to fall on *m*.

Another rod, *q*, also more than balanced by a ball, *r*, was insulated by a glass tube, *s*, and connected with the quadrant-electrometer by a very fine wire. It could also turn around a pivot at *t*; so that when the ring *u* rested upon it, it fell on the upper condenser-plate *a*, and connected it with the electrometer: when the weight *u* was raised by the crank *v*, the rod rested against *f*, and so connected the electrometer to the earth, to which the other quadrants were already connected.

At the beginning of an experiment, the insulating plate to be tested having been placed between the condenser-plates *a* and *b*, the handle was brought into such a position that the ring, *d*, rested on the plate around *a*. The lengths of the threads between *d* and *f* were such that *o* for this position of the handle did not touch *w*, and so *h* remained in connexion with the earth; and so *d* was also connected with the earth, and thus also with *b*. On now turning the handle further, the ball *h* descended to the ball *m*, and thus charged the condenser for any time desired. On now reversing the motion, the following operations took place:—

First, the ball *h* rose and discharged the condenser.

Second, the guard-ring *d* ascended.

Third, the rod *q*, which had been previously in contact with *p*, thus bringing the quadrant-electrometer to zero, now moved down and rested on the upper condenser-plate *a*. Thus any return charge quickly showed itself on the electrometer. The amount of deflection of the instrument depends upon the character of the dielectric, its thickness, the charge of the battery, the time of contact with the battery, and upon the length of time of discharging.

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III.

In comparing the glass with the crystal plates, the electrometer was rendered as little sensitive as the ordinary arrangement of the instrument without the inductor-plate would allow. The electric absorption of the glass plates for a charge in the battery of two or three sparks from the Harris unit-jar then sufficed, after 20 or 30 seconds contact with the battery and 5 seconds discharging time, to give a deflection of about 200 scale-divisions, which were millimetres. The quartz and calcite plates were then alternately substituted for the glass, the same charge and the same intervals of contact being used, and the resulting deflections noted—two plates of each substance of the same thickness being used.

The results of the measurements are given in the following Tables, the effect of the glass being called 100.

TABLE I.

(*a*).

April 12, 1880.
Charge of battery, 2 sparks.
Contact 30 seconds.

Glass (1st plate)	100·0
Quartz (1st plate)	17·1
,, (2nd plate)	20·0
Calcite (1st plate)	0·0
,, (2nd plate)	0·0

(*b*).

April 13, 1880.
Charge of battery, 3 sparks.
Contact 20 seconds.

Glass (1st plate)	100·0
Quartz (1st plate)	19·3
Calcite (1st plate)	0·0

(*c*).

April 14, 1880.
Charge 3 sparks.
Contact 10 seconds.
Plates carefully dried by being in desiccator over night.

Glass (1st plate)	100·0
Quartz (1st plate)	10·7
Calcite (1st plate)	0·0

(*d*).

April 22, 1880.
Charge 2 sparks.
Contact 30 seconds.
Plate in desiccator since April 14.

Glass (2nd plate)	100·0
,, (1st plate)	96·3
Quartz (1st plate)	13·4
,, (2nd plate)	12·1
Calcite (1st plate)	0·0
,, (2nd plate)	0·0

TABLE II.

May 1. Relative Effects for different Intensities of Charge and Time of Contact.

Charge of battery.	Material.	Deflections, in millimetres.		
		Contact 5 seconds.	Contact 10 seconds.	Contact 20 seconds.
One spark	Glass (1st)	133·0	189·3	225·0
	Quartz (1st)	13·0	22·7	34·3
	Calcite (1st)	0·0	0·0	0·0
Two sparks	Glass (1st)	Off the scale	Off the scale	Off the scale
	Quartz (1st)	24·0	35·0	50·0
	Calcite (1st)	0·0	0·0	0·0

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These Tables seem to prove beyond question that calcite in clear crystal has no electric absorption. Quartz seems to have about $\frac{1}{8}$ that of glass; but we have remarked that quartz is not a good substance to test the theory upon.

Some experiments were made with cleavage-plates of selenite, which are always more or less imperfect, as the laminæ are very apt to separate. These gave, however, effects about $\frac{1}{3}$ or $\frac{1}{4}$ those of glass.

In order to test still further the absence of electric absorption in calcite, the electrometer was rendered very sensitive, and the calcite plates were tested with gradually increasing charges, from that which in glass gave 200 millim. after 1 second contact, up to the maximum charge (ten sparks of the unit-jar) which the condensers were capable of carrying. In these trials, the calcite still showed no effect, even with 30 seconds contact. During these experiments glass was frequently substituted for the calcite, to leave no question but that the apparatus was in working order.

It is to be noted that the relative effects of the quartz and the glass were different for dried plates and plates exposed to the atmosphere. This was possibly due to the glass being a better insulator, and thus retaining its charge better when dry than in its ordinary condition.

IV.

Thus we have found, for the first time, a solid which has no electric absorption; and it is a body which, above all others, the theory of Clausius and Maxwell would indicate. The small amount of the effect in quartz and selenite also confirms the theory, provided that we can show that in the given piece of quartz some molecules of right-handed quartz were mixed with the left; for we know that the theoretical conditions for the absence of electric absorption are rarely satisfied by laminated substances like selenite or mica. If the theory is confirmed, the apparatus here described should give the only test we yet have of the perfect homogeneity of insulating bodies; for any optical test cannot penetrate, as this does, to the very structure of the molecule.

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LVII. *On the Beats of Consonances of the Form* h : 1.

By R. H. M. Bosanquet*.

[Plates IV.–VII.]

Ohm's Law, and the Hypothesis of Resultant Displacements.

1. THE doctrine known as Ohm's law states that the simplest form of motion by which definite musical pitch is defined to the ear is the pendulum-vibration. It may be extended as follows:—In all cases in which Ohm's law holds, the ear resolves any complex of two or more simultaneous pendulum-vibrations into the original pendulum-vibrations of which they consist, and hears them as distinct and independent sounds.

I rest my belief in Ohm's law mainly on ordinary phenomena, not on refinements or difficult observations; and I shall endeavour to make this my course throughout.

2. So long as, in our mechanical arrangements, we approximate more and more nearly to the condition of things by which we know that simple harmonic vibrations must be produced, we also approximate in the character of the resulting sound to a pure and simple note of definite pitch, free from harmonics and other accompaniments.

So far as simple sounds are concerned, therefore, we receive Ohm's law as being at all events approximately true generally, and in all probability absolutely true when sounds of small intensity alone are considered.

3. When two different sounds are heard together, we have phenomena of which the following is a slight sketch.

If the two sounds are very nearly of the same pitch, they are not heard according to Ohm's law, *i. e.* separately and independently, but in the form of resultant displacements. The most important case is that in which the two sounds are of nearly equal intensity. In this case one sound is heard, intermediate in pitch between the two primaries, and oscillating in intensity between a certain maximum and nothing. These oscillations are what are called the beats of imperfect unisons. Now, as the two notes separate from one another in pitch, the character of the phenomenon changes; and at a certain point the two notes begin to be heard separately and independently, beside the beats which accompany them. It is this phenomenon that is accounted for by Helmholtz's theory of the existence of vibrating bodies, in the ear, having sympathy of a certain definite degree with the various notes.

4. Helmholtz ascertained the approximate degree of this

* Communicated by the Physical Society.

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sympathy by considerations of a somewhat indirect character. I wish to point out the important bearing, on the theory, of the direct determination of the interval which separates the region in which two notes are heard only as resultant displacements, from that in which they begin to be heard separately, in accordance with Ohm's law.

5. The experiments I have made on this point have been mostly conducted by means of my enharmonic organ, in which I have a collection of notes separated for the most part by single commas.

The results, so far as I have gone, are:—1. The critical interval, at which two notes begin to be heard beside their beats, or resultant displacements, is about two commas, throughout that medium portion of the scale which is used in practical music. 2. This critical interval appears not to be exactly the same for all ears. In my own case two notes two commas apart are not heard distinctly beside the beats. In the case of Mr. Parratt, who has kindly examined the point with me, two notes two commas apart are distinctly heard beside the beats. In both cases the beats alone are heard with an interval of one comma, and the two notes are quite clear beside the beats with an interval of three commas.

I propose to undertake further experiments, with the view of determining this initial interval more accurately. So far as the above results go, they are quite consistent with Helmholtz's assumption as to the degree of sympathy of the ear.

6. Independently of any theory, the fact that at a certain point the ear begins to separate out two independent pendulum-vibrations from the resultant displacements, is one that must be recognized. It is easy to show that it is inconsistent with that representation of facts which assumes that beats arise only from the resultant forms exhibited by the superposition of the two vibrations on one receptive mechanism. This is shown as follows.

7. If we combine two vibrations of equal amplitude, which we may take $=1$, $\cos pt$ and $\cos(qt-\epsilon)$, on the same receptive mechanism, the effect is to produce a resultant displacement represented by

$$2\cos\frac{(p+q)t-\epsilon}{2}\,.\,\cos\frac{(p-q)t+\epsilon}{2}.$$

This would be heard, by a hypothetical ear receiving the whole disturbance on the same sensorium, as a note whose frequency is the arithmetic mean between the frequencies of the two primaries, and having oscillations of intensity whose frequency is defined by a pendulum-vibration of frequency equal

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to half the difference of the frequencies of the primaries. This is what is actually heard in the case of two notes less than two commas apart.

8. When the interval is greater than two commas, this ceases to represent the whole phenomenon perceived by the ear as it exists. The separate notes step in beside the resultant form represented by the above expression, with its beats and note having the frequency of the arithmetic mean. As the interval increases, the separate notes become more and more prominent, and the beats diminish in loudness and distinctness, till, by the time that a certain interval is reached, which is about a minor third in the middle of the scale, the beats practically disappear and the two notes alone survive.

9. It has been supposed by some that the beats disappear only in consequence of their rapidity*; and it is clear that under this supposition, as ordinarily made, lies the assumption that the mass of tone continues to be received in the same manner all the time—*i. e.* that the phenomena of the beats of imperfect consonances and combination-tones are to be explained by reasoning analogous to that of the above formula, which supposes the whole displacement reduced to its resultant on one receptive mechanism. This, for instance, is assumed whenever Smith's or Young's theories of beats are admitted as sufficient explanations of the phenomena.

10. In such cases, (*a*) it is forgotten that the fundamental assumption carries another consequence with it than those it was desired to explain; (*b*) the explanation itself also fails in an important point.

(*a*) The other consequence is, that if it were true that the receptive mechanism of the ear received a resultant displacement, so that the combination was as represented by the above formula, then the primary notes would not be heard at all, and the note that would be heard would have the arithmetic mean of the frequencies of the primaries.

E. g., in the case of a fifth (4 : 6) the note heard would be the major third (5), which would beat very rapidly; just as, when I myself hear the resultant of notes two commas apart, it is one note midway between them beating rapidly. But, as a matter of fact, the note 5 is not heard at all in the above case.

(*b*) Again, supposing that in some unexplained way the beats whose speed is $\frac{p-q}{2}$ in the above notation gave rise to a note, as supposed by König. Then the speed of that note

* This is absolutely disproved by the argument in Helmholtz's *Tonempf.* p. 286, ed. 4.

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does not agree with that required for König's first beat-note, which has the same speed as Helmholtz's difference-tone, or $(p-q)$ in the above notation.

11. The relationship of these resultant displacements to the phenomena in the general case, is most conveniently studied by means of the curves drawn by Donkin's harmonograph. The instrument in my possession has a rather restricted number of change-wheels; and one of my first tasks in the St. John's College laboratory has been to cut additional change-wheels for this instrument, for the purpose of illustrating this subject graphically*. (See Plates IV. to VII.)

12. The examination of the curves leads us to the following conclusions.

In every case, whether of beats of unisons, or of beats of imperfect consonances, the examination of the curves shows a portion of a harmonic curve lying through the vertices of the single resultant vibrations, which portion corresponds in duration to the beats as given either by Smith's rule or the ordinary rule for beats.

The durations of these harmonic curves are different in different cases. Three principal types may be distinguished:—

Let E, F be the amplitudes; $p : q$ the ratio in lowest terms of the exact consonance whose small variation is considered $(q > p)$.

(1) If E | p is considerably less than F | q, there are q complete harmonic curves both at top and bottom, and the duration of each is q times that of the Smith's beat.

(2) If E | p = F | q, there are $p+q$ complete harmonic curves which may be called external, passing both top and bottom, and the duration of each external curve is $p+q$ times that of the Smith's beat; also there are $q-p$ internal curves, which lie nearer the middle; the duration of each internal curve is $q-p$ times that of the Smith's beat.

(3) If E | p is considerably greater than F | q, there are p harmonic curves both at top and bottom. They are not complete, but appear to form portions of curves of long period.

13. In all cases the curves which correspond to the beats, as ascertained by Smith's method or the ordinary formula, lie like series of bows, one series at the top and the other at the bottom.

The complete period of the pendulum-vibration, of which each of these bows forms a part, is always longer than the single bow or Smith's beat, according to the above rules.

14. Now, according to a well-known principle of mechanics, no pendulum-vibration can give rise to one of another period,

* These curves are of such interest that I devote some space to their discussion, § 77 &c.

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in a system in which the forces are proportional to the displacements.

15. In the present case, if we find a term present whose whole period is that of the Smith's beat, it must therefore arise by transformation, *i. e.* through the presence of terms of higher orders than the first. We shall use generally the expression "transformation" to signify the effect on a system of terms higher than the first in the expression for the forces. This is substantially Helmholtz's explanation of the difference-tone, which is identical with the lowest beat-note of König.

16. We shall show that all König's beat-notes can be accounted for in a similar manner, by the assumption that terms of higher orders become important in the mechanism of the ear when the displacements are considerable.

17. We can illustrate further the difference produced in the curves by the admission of the difference-tone or beat-note as part of the mass of sound. The characteristic difference is, that the medial line is itself bent into a curve, whose whole period is that of the Smith's beat. I have not been able to draw any long curve to show this; but the appearance of the curve at the top of illustration A (p. 425) is very like it in a general way. This illustration represents the square term of the force developed by a fifth (2 : 3) in a transforming system. B is the figure of the total disturbed force in a similar case; but the throwing-up of the medial line is not so prominent as it would be in a longer curve. I have, however, no machine that will draw the combination; and the construction of a long curve of this kind is not worth the labour it would entail.

18. We sum up this part in the following conclusions:—

As two notes of equal amplitudes separate from unison, they are at first received by the ear in the manner of resultant displacements, consisting of the beats of a note whose frequency is midway between that of the primaries.

When the interval reaches about two commas, the ear begins to resolve the resultant displacements, and the primary notes step in beside the beats.

When the interval reaches a minor third in the ordinary parts of the scale, neither the beats nor the intermediate pitch of the resultant note are any longer audible, at least as matter of ordinary perception; but the resultant displacement which reaches the ear is decomposed, and produces the sensation of the two primary notes, perfectly distinct from each other: that is to say, Ohm's law has set in, and is true, for ordinary perceptions and in the ordinary regions of the scale, for the minor third and all greater intervals.

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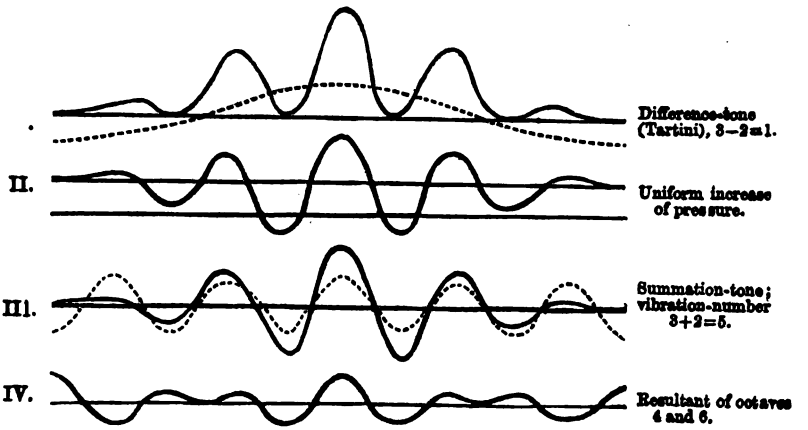
A.

Statical Analysis of Disturbance proportional to Square of Displacement, showing origin of the combination-tone. Fifth 2 : 3.



I. From disturbance proportional to square of displacement subtract simple tone. Amplitude ½ of disturbance. Period = that of the beat. (Vibration-number = difference of vibration-numbers, 3−2=1.)

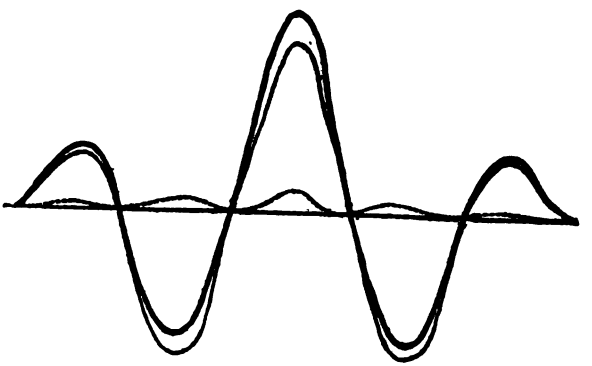
II. Subtract ½ of amplitude of disturbance.

III. Subtract a simple tone. Amplitude ½ of that of disturbance. Period = ½ that of resultant vibration in original beat. (Vibration-number = sum of vibration-numbers of original tones, 3+2=5.)

IV. There remains a succession of beats like the original beats of the fifth, an octave higher, *i. e.* the combination of the octaves of the original notes.

B.

Beat of Fifth, 2 : 3. *Disturbed by term proportional to square of displacement, showing origin of the combination-tone.*



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19. We may notice here incidentally that it is necessary that the resolved primaries should be uniform and steady, in order that the beats exhibited in the resultant forms may retain their regularity. Those who support the Young-and-Smith theory generally have a sort of confused idea that the primaries are modified when superposed into their resultant.

20. How, then, do the beats of mistuned consonances arise? They may be regarded as springing from interference of new notes, which arise by transformation, in the passage of the resultant forms through the transmitting mechanism of the ear, before the analysis by the sensorium.

Experiments.

21. The engine and bellows* being adjusted to run continuously and quietly, I began to follow the course of König's experiments at the point where he deals with the combinations of the note C, following his form not accurately, but with such divergences as the difference in the apparatus suggested. After going through one or two sets in the way hereinafter described, I concentrated my attention on the analysis of beats, and specially on those of mistuned consonances of the form $h : 1$. It will be seen that after a time I entirely discarded resonators, having convinced myself that, so far as they were concerned, the beats of mistuned consonances, other than unisons, with the beat-notes, difference- and combination-tones of all orders, and, in fact, all that I had to observe, were of a purely subjective nature, and were extinguished by resonators properly used, so far as my arrangements enabled me to perceive.

22. The mode in which I then pursued the observations on the beats of the mistuned consonances in question was, to adjust the notes and leave them sounding uniformly and continuously by the hour together. I then walked about the room listening to the combination in all the various forms in which it presented itself, went outside and came in again, always keeping in view the question, what are the sounds that these beats consist of?

23. It is hard to believe that a question to which the answer is tolerably simple could be so difficult. Yet it is *very* difficult; it is one of the most difficult things I ever tried to do, to analyze these apparently complicated sounds into their elements by the ear alone. And when I state my results, I must not be taken to mean that the elements I mention are all that are present. In fact, one of the great difficulties is that there appear to be such a number of different sounds.

* See Phil. Mag. Oct. 1880.

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Some of these are probably due to the imagination; others probably exist in small intensity. And I am satisfied that there still exists a large field of work in the further prosecution of this subject. But of the main result I have no doubt whatever; and that is:—

24. The beats of mistuned consonances of the form $h : 1_1$, where h is nearly some whole number, consist mainly of variations of intensity of the lower note when the beats of the harmonics are eliminated.

25. I was prepared for this result in the case of the octave by my preliminary experiments (Phil. Mag. viii. p. 293), but did not proceed further till I had verified it and got my ear to perceive it readily under the new conditions, which required two or three days. I then got Mr. Parratt to come and listen. He was much disturbed by the trifling noises from the engine, belts, &c.; and I blew the bellows myself for a time. Eventually he came to the same conclusion, but with an amount of hesitation and difficulty which showed me what an important element practice is in these observations.

26. I then started these observations with the mistuned twelfth, proceeding in the same way. I seemed to have the same difficulty as before in seizing the phenomenon; and when I eventually decided that these beats were also on the lower note, it was not in pursuance of any preconceived conclusion; for I had no idea at that time of the explanation I now give, and certainly none of the presence of the second difference-tone, or its identity with one of König's beat-notes.

27. Having got so far, I found the remaining verification, of the beats of the mistuned double octave, somewhat easier. These are also on the lower note when they are heard. I have never heard the beats of a mistuned consonance with any wider interval, with the notes I employ, as clear and unmistakable phenomena. Such beats may be discernible by more acute ears, or with notes of a more powerful quality, as they were discerned by König. But in such cases it will be incumbent on the observer to purge the beats from the suspicion of containing the beats of harmonics, as I have done.

28. Mr. Parratt subsequently convinced himself, as before, that the beats of the twelfth and double octave were all heard on the lower notes. I endeavoured, as far as possible, to make his observations independent by avoiding communicating my conclusions to him beforehand.

29. The elimination of the beats of the harmonics depends on the following considerations. The notes employed were examined, with and without resonators, as to the presence of harmonics. These, so far as they are objective, are readily

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detected with resonators. The beats of the harmonics, where they existed objectively, were also examined with resonators. After a little practice the sound of these beats became familiar enough to prevent their being confused with the beats of the low notes, and the two sets of beats could be observed independently.

30. The only harmonic that exists in these notes in sensible intensity is the twelfth; and this does not appear to originate in the same manner as the principal note. It is heard separately, as it were, and as if it had an independent origin. It seems probable that it arises in connexion with the movements of the air about the mouth-piece, and not by resonance in the cavity of the bottle, like the principal note. At all events, whatever the cause may be, the effect is that the presence of this note is readily distinguished and allowed for, and there is no risk of its being mixed up with the rest of the phenomenon.

31. The notes employed are of moderate strength. It seems to me that the employment of notes of great power is open to the objection that it introduces all sorts of transformations depending on the greatness of the diplacements; and in this respect alone König's procedure is open to considerable objection. I have confined myself to notes of moderate strength, lying in those regions of the scale which are in ordinary use in music. It is phenomena thus presented that we really seek to understand; and I do not think that any thing is gained by pushing the investigation into those extreme regions where it is possible and highly probable that the ordinary laws of hearing become modified.

32. The first series of notes examined in the above manner were the set of pairs

C : *c*
C : *g*
C : *c′*

C : *e′*
C : *g′*
C : *c″*

The beats produced by mistuning, when cleared of the harmonic beats, were heard only in the first three cases.

The second set of pairs was

c : *c′*
c : *g′*

---

c : *c″*
c : *e″*
c : *g″*

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The beats in question were only heard in the first two cases.

The third set was

$$\frac{c' : c''}{c' : g''}$$
$$c' : c'''$$

The beats in question were only heard in the first case.

33. In the few experiments hitherto made with notes of higher pitch, the beats of mistuned consonances of the form $h : 1$ were not heard when the beats of the harmonics were eliminated, unless the power of the notes was very greatly increased. In this region, however, König's own observations are very full and complete.

34. We notice at once the decrease in the range within which the phenomena are heard as we rise in the scale. This is at once accounted for on the hypothesis of transformation, by the consideration that the displacements to which the higher notes give rise are much smaller than those of the lower notes. If we knew the law of the decrease, we might obtain a relation between the coefficients of the different terms in the expression for the character of the transforming mechanism. König has attempted to formulate a law of decrease; and I have done so on a previous occasion; but this part of the subject is as yet too hypothetical to admit of satisfactory treatment.

Objective and Subjective Phenomena.

Resonators.

35. On beginning work I endeavoured, in the first instance, to ascertain what evidence resonators are capabable of furnishing as to the nature of binary combinations. There are a few points connected with their use which require attention.

36. I have always found difficulty in getting results of a definite character with resonators, whether applied directly to one ear in the manner described by Helmholtz, or connected with one ear by means of a flexible tube, as practised by others. There are three difficulties which occur: (1) pressing the tube or orifice into the ear is apt to close the inner passage of that organ; (2) if the tube or orifice is applied lightly, it does not completely occupy the passage, and external sound comes past it into the ear; and (3) it is impossible so to stop the unused ear as to prevent the external impressions from arriving there and causing confusion.

37. The method I ultimately adopted was as follows:—A copper tube of $\frac{1}{4}$ inch diameter was bent into a semicircle, the diameter of which was nearly 8 inches. At the middle of the tube, and at right angles to its plane, another copper tube was

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inserted, 2 inches long, which tapered down to an orifice $\frac{1}{8}$ inch in diameter; this served to communicate with the interior of a resonator by means of a small flexible tube. The extremities of the semicircle were turned inwards and upwards; and into them two brass tubes were inserted, $\frac{3}{4}$ inch long and $\frac{1}{8}$ inch in internal diameter, screwed on the outside. Over each of these was fitted a brass tube, screwed inside, carrying an ivory nipple, such as is used for ear-trumpets. I generally covered the nipple with a couple of thicknesses of thin india-rubber tube.

38. When used, the semicircle is passed under the chin with the resonator-attachment projecting in front. The nipples are at first screwed back as far as possible, brought opposite the orifices of the ears, and then screwed forward until they enter the ears. They are then gradually advanced until the passages are closed to external sounds. Something depends on the way the tube is held. With practice it is possible to hold it so that the passages are closed to external sounds without screwing the nipples in very tight. When they are screwed very tight, it is rather unpleasant, and even painful. But it is necessary constantly to be on one's guard against being deceived by an occasional entrance of external sounds if the nipples are not quite tight. This instrument was made for me some time ago by Mr. Walters of Moorgate Street; it has already been described (Proc. Mus. Assoc. 1879–80, p. 18).

39. The resonators I employ are bottles fitted with corks having apertures of various sizes. I sometimes tune them with water, in the same way as the bottle-notes; sometimes I insert tubes into the apertures to lower the pitch. A bit of small glass tubing passed through the cork is connected by an india-rubber tube with the above-described ear-piece.

40. By means of these arrangements I some time ago examined the nature of the ordinary first difference-tone, and convinced myself that it is not capable of exciting a resonator (*l. c.* p. 20). This conclusion has also been arrived at by others*. In short, the difference-tone of Helmholtz, or first beat-note of König, as ordinarily heard, is not objective in its character. It is therefore subjective. (See Helmholtz, *Tonempfindungen*, 4th ed. p. 259.) In making the experiment of listening for the difference-tone through a resonator, it is necessary to be careful that the ears are both closed to external sounds; otherwise the external notes will penetrate through, the difference-tone will appear, and the completeness of the cut-off effected by the resonator will be entirely lost.

* Preyer, *Akustische Untersuchungen*, p. 13.

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41. When beginning the regular course of experiments according to the general outline of König's work, I was careful, in the first instance, to examine the various masses of sound presented, with resonators arranged as above indicated. In examining, for instance, the intervals made by the note C with the various notes of the octave above it (up to *c*), I first fixed the resonator at some one pitch, and then ran the movable note up through the octave. Then, as this did not seem a good process for analysis, I set the mistuned octave beating, or any other combination it was desired to examine, and ran the pitch of the resonator up and down with water to see if any thing could be detected. And here I came across an observation that puzzled me for some time.

42. Suppose the mistuned octave C : *c* was sounding, and I examined the lower note with the resonator: sometimes it appeared loud and steady, at other times as if beating powerfully. On removing the resonator-attachment from the ear, the lower note was always heard to beat powerfully. The explanation was simple. When the nipples of the resonator-attachment fitted tightly into the ears, nothing reached the ear but the uniform vibrations of the resonator sounding C. But if there was the slightest looseness between the nipple and the passage of either ear, the second note (*c*) of the combination got in, and gave rise to the subjective difference-tone (first beat-note of König), by interference of which with the C I explain the beats on that note. *These beats are therefore subjective.*

43. A considerable number of combinations, including examples of the principal forms of beat, rattle, or roll, were examined in this way; and when the precautions above indicated were attended to, the results were in all cases to negative the objective existence of all forms of beats, and beat-notes or difference-tones, except the beats which arise from the interference of approximate unisons, which beats arise from both notes acting on the resonator simultaneously. This of course includes the beats produced by objective harmonics.

Course of General Experiments.

44. The following is the detailed examination of the combinations of the note C, made in a continuous and connected manner. The results have a general correspondence with those of König. The numerous rattles and rolls of beats mentioned were not further analyzed for the most part: the analysis of these is very difficult; and, as has been already stated, a separate investigation is required in every such case. Some attention was devoted to beats of the mistuned fifth, both in

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the case mentioned and in others; but no final result was arrived at. In two different cases of mistuned fifths (2 : 3, nearly), I had a strong impression that the note 7 formed an important part of the beat. This would be a summation-tone of the second order, thus $2 \times 2 + 3$. I am confident that it did not arise from harmonics.

These experiments were made after some experience had been gained.

C_1 : C.

Rattle up to

C : F.

Slow beats up to

C : G, smooth fifth. Roll only perceptible when the ear is held close to the two sources of sound.

——, 5 beats sharp. Perception of pitch very difficult in this part of the scale. There is a heavy beat like a knock, which appears to affect the whole mass of sound*. The low beat of C_1 : C is only distinguishable with difficulty, or hardly at all.

(Another occasion.) Mr. Parratt describes the fifth C : G, beating slowly, as consisting of E♭ and C_1 : C in addition to the primary notes; the mass of the beat is at least partly on E♭. I do not hear the E♭, but seem to hear the note E.

(Another occasion.) Mr. Parratt is clear that the beat of the mistuned fifth C : G is on C_1 : C alone; but he still hears the E♭ in the mass of tone. I seem to hear the beats both on C and C_1 : C; but I have a difficulty in separating the octaves in this deep pitch.

C : G, 8 beats sharp. Clear rattle, with suspicion of roll beside it.

—— 10 beats sharp. Beats just distinguishable. Roll.

C : B♭. Rattle emerges.

Below

C : *c*, 8 beats can be counted.

——, 4 beats very distinct. Consist entirely of variation of intensity of lower note. This effect is very clear and remarkable.

——, a very slow beat flat. Here it was easy to recognize the effect of the shift of phase in the apparent great increase of volume of the lower note at one period of the change. The upper note was not perceptibly affected.

C : *c*. A slight rich roll with smooth tone. The production of the roll depends a good deal on the phase, as is seen by leading up to *c* with a very slow beat.

* I take this entry to show that no progress had been made with the resolution of the phenomenon into its elements.

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The twelfth of the C was plainly distinguishable, but it appeared to keep separate from the mass of tone; it was perfectly steady and unaffected by combination with *c*.

C : *c*, 2 beats sharp. Phenomena undistinguishable from 2 beats below.

——, 4 beats sharp. Perhaps a little less roll in the strong part of the beat.

——, 8 beats sharp. The mass of the beats is of pitch near C; but the exact pitch is very difficult to distinguish. It is a deep heavy rattle, quite distinct in pitch from the upper note.

C : *e*. If there is any slow beat in passing through this, it is very difficult to distinguish. I am inclined to negative it.

C : *f*♯. Roll.

Slow beats up to

C : *g*. These beats consist of alternations of intensity of C. They are more difficult to count than those of C : *c*. I counted them at 5 below.

——. Slow beats above.

C : *b*♭. Rattle, turning into beats easily counted at 4 below *c′*. These beats also consist of variations of intensity of the lower note.

C : *c′*.

The beats above *c′* were also counted at 4 above, while the engine was going, without difficulty.

45. Above this, in the neighbourhood of the binary consonances C : *e′* &c., I have never been able to obtain slow beats in such a way that they could be readily perceived (even without the engine) or certainly counted.

46. The mode adopted to examine cases in which the beats could not be perceived was, to introduce a third note, such as *c′*, which gave beats with the C, and tune it true. Then any note, such as *e′* or *g′*, could be readily tuned so that the whole three notes gave 1, 2, 3, or 4 beats. When this had been done, the intermediate note *c′* was removed. If the pair examined was capable of giving beats at all, they should then have been audible.

47. The details of the above course furnish no new results; I have not, therefore, thought it worth while to give similar courses for other sets of notes. Those results which are worthy of mention have been already stated.

Theory of the Beats of Mistuned Consonances of the form h : 1.

48. Let *n* be the frequency of the lowest note, *m* the number

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of beats per second. Then the mistuned octave is $n : 2n \pm m$; the mistuned twelfth is $n : 3n \pm m$; and so on.

49. Beats of the mistuned octave,

$$n : 2n \pm m.$$

Number of beats $= m$.

m variations of intensity of the lower note (n) are produced by interference of notes n and $n \pm m$; and $n \pm m$ is the first combination-tone (difference-tone of form $p-q$), of the primaries n and $2n \pm m$.

This rests chiefly on the observation that the beats, when the octave harmonic is eliminated, consist entirely of variations of intensity of the lower note.

The existence of the first combination-tone in question ($p-q$) is well known. It is easily demonstrated in the neighbouring case of intervals not far removed from the fifth, when the beats of the first two combination-tones are specially prominent (secondary beats of König).

50. Beats of the mistuned twelfth,

$$n : 3n \pm m,$$

Number of beats $= m$.

m variations of intensity of the lower note (n) are produced by interference of notes n and $n \pm m$. And $n \pm m$ is the second combination-tone (difference-tone of form $2p-q$) of the primaries n and $3n \pm m$.

This rests also chiefly on the observation that the beats, when the third partials are eliminated, consist entirely of variations of intensity of the lower note.

The existence of the second combination-tone in question ($2p-q$) is demonstrated in many cases by König. It is easily heard in the case of intervals near the octave high in the scale. It is also easily detected by the secondary beats which it forms with the first combination-tone in the case of intervals near the fifth—also less easily by the secondary beats which it forms with the third combination-tone in intervals near 2 : 5, at which point the second and third combination-tones coincide.

51. Beats of the mistuned fifteenth or double octave,

$$n : 4n \pm m.$$

Number of beats $= m$.

m variations of intensity of the lower note (n) are produced by interference of notes n and $n \pm m$. And $n \pm m$ is the third combination-tone (difference-tone of form $3p-q$) of the primaries n and $4n \pm m$.

This rests also chiefly on the observation that the beats, when

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the fourth partials are eliminated, consist entirely of variations of intensity of the lower note.

The existence of the third combination-tone in question $(3p-q)$ is demonstrated in many cases by König. It is heard not so easily as the lower combination-tones, in the case of intervals near the twelfth high in the scale. It is also less easily detected by the secondary beats which it forms with the second combination-tone in the case of intervals near 2 : 5, at which point the second and third combination-tones coincide—also much less easily by the secondary beats which it forms with the fourth combination-tone in the case of intervals near 2 : 7, at which point the third and fourth combination-tones coincide.

52. Beats of the mistuned tierce (two octaves and a major third),

$$n : 5n \pm m.$$

These beats are much less easily detected in pure notes of the ordinary strength than any of the foregoing. They are recorded by König; but I have never heard them clearly. As it is certain that König's notes were not perfectly pure, and he does not analyze the beats, we cannot tell whether the variations of the lower note were produced in his experiments. If they were, they are to be accounted for in a similar manner.

Number of beats $= m$.

m variations of intensity of lower note (n) are produced by interference of notes n and $n \pm m$. And $n \pm m$ is the fourth combination-tone (difference-tone of form $4p-q$) of the primaries n and $5n \pm m$.

The existence of the fourth combination-tone in question $(4p-q)$ is demonstrated directly by König in the case of intervals near the double octave $c''' : c^{v}$. It is also less easily detected by the secondary beats which it forms with the third combination-tone in the case of intervals near 2 : 7, at which point the third and fourth combination-tones coincide.

53. Beats of the mistuned consonance of the nineteenth are recorded by König;

$$n : 6n \pm m.$$

Number of beats $= m$.

m variations of intensity of lower note (n) might be produced by interference of n and $n \pm m$. And $n \pm m$ is the fifth combination-tone (difference-tone of form $5p-q$) of the primaries n and $6n \pm m$.

The existence of the fifth combination-tone in question $(5p-q)$ is not anywhere directly demonstrated. Secondary beats, which might be produced by its interference with the

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fourth combination-tone, are recorded by König in the neighbourhood of the interval $c : d''$

54. Beats of the mistuned consonance 1 : 7 are recorded by König. These might be produced by a sixth combination-tone (difference-tone of form $6p-q$) of the primaries n and $7n \pm m$.

55. Beats of the mistuned consonance 1 : 8 are recorded. These might be produced by a seventh combination-tone (difference-tone of form $7p-q$) of the primaries $8n \pm m$.

56. As far as my own experience goes, however, I have no direct and palpable evidence of beats of mistuned consonances higher than 1 : 4, or of the existence of combination-tones higher than the third ($3p-q$) in recognizable intensity. Up to this point the phenomena are quite clear; and there is no possible doubt as to their nature.

But in considering these limited results it must be remembered, (1) that I have restricted myself to notes of very moderate intensity, so that the phenomena might correspond as nearly as possible to those which are presented to our ears in practice, and (2) that, although I was unable to get rid entirely of the presence of upper partials in all cases, yet the phenomena were subjected to a careful and prolonged analysis by listening under varied conditions, until the effect of the upper partials could be separated out and eliminated with certainty. And we have at all events no security that these upper partials did not give rise to many of König's results; indeed it is almost certain that they must have entered into those results.

Note.—The present paper was written before the appearance of König's paper in Wiedemann's *Annalen* in the present year. The discussion of that paper, though necessary for a complete view of the subject, must be reserved till after the conclusion of the present paper.

[To be continued.]

LVIII. *An Analysis of Relationships.*

By A. Macfarlane, *M.A., D.Sc., F.R.S.E.**

IN this article I propose to describe some results of several papers on an Algebra of Relationship, which I have recently contributed to the Royal Society of Edinburgh†. The Logic of Relatives‡ has been worked at by De Morgan, Leslie Ellis, Harley, and C. S. Peirce§; and the last-named

* Communicated by the Author.

† Proc. Roy. Soc. Edinb. May 1879, Dec. 1880, and March 1881.

‡ Since writing this article I have had the opportunity of reading two interesting and suggestive papers on the Logic of Relatives, by Mr. J. J. Murphy.

§ For references see Jevons's 'Principles of Science,' p. 23.

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philosopher has recently published* the first part of a memoir containing a summary of his investigations. I have not taken up the subject of relation in general, but have restricted myself, in the first place, to a well-defined and important part, namely the relations of men due to consanguinity and affinity.

The particular class of objects considered in the investigation is in its widest extent the natural class *mankind*, by which term I mean the entire number of men who have existed, exist, or will exist. The universal properties of the symbols are based on the universal properties of the class. For particular investigations we may limit in any manner the collection of men considered—for example, to the inhabitants of Christendom, or the subjects of Queen Victoria, or the citizens of a given town, or the members of a given household.

Let U denote any person in the collection of men considered; then U_A is an appropriate mathematical expression for the person whose name is A, and U_B for the person whose name is B. For the sake of shortness U_A may be written A. Also ΣU is an appropriate mathematical expression for all the persons in the collection.

Let cA be used to denote any child of A; then $\frac{1}{c}$A denotes either parent of A. To express a certain child of A, two children of A, three children of A, &c., we require $1c$A, $2c$A, $3c$A, &c. The completeness of a number may be indicated by a dot over the number, as $\dot{3}c$A, the three children of A. When the value of the complete number is not expressed, the expression takes the form ΣcA, all the children of A. Subscript numbers, as in c_1A, c_2A, &c., are the appropriate mathematical symbols for expressing the eldest child of A, the second child of A, &c.

Since cA denotes any child of A, cc A will denote any child of any child of A, hence any grandchild of A. Similarly, $c\frac{1}{c}$A will denote any child of either parent of A—that is, any brother or sister of A, or A himself (or herself). Also $\frac{1}{c}c$A will denote either parent of any child of A, hence any consort of A or A himself (or herself); and $\frac{1}{c}\frac{1}{c}$A will denote either parent of either parent of A—that is, any grandparent of A. The expression $\frac{1}{c}$A may be denoted by c^{-1}A; then the above are denoted by c^2A, c^{1-1}A, c^{-1+1}A, c^{-2}A respectively. The ex-

* American Journal of Mathematics, vol. iii. p. 15.

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pressions c^{1-1} and c^{-1+1} may or may not reduce to c^0, which means *self*.

Relationships expressed in terms of c and c^{-1} take no account of distinctions due to difference of sex; they may therefore be called general relationships, in contrast to the specific relationships into which they are broken up by the introduction of distinctions of sex. Let the order of any general relationship be defined as the number of times c enters into its expression, whether directly or inversely. The relationships of the $(n+1)$th order are derived from those of the nth order by prefixing c and c^{-1} before each of the latter. In the case of the first order c and c^{-1} are applied to 1. Hence, in order to obtain all the relationships of the nth order, we have merely to expand $(c+c^{-1})^n$, but in such a manner that the order of the factors in each term is preserved. It is evident from the mode of development, that the number of general relationships in the nth order is 2^n. Appended is the first portion of the development.

General Relationships.

Order.	Expression.	Meaning, if reducible.	Meaning, if irreducible.
0	c^0.	.....	Self.
I.	c^1.	.....	Child.
	c^{-1}.	.....	Parent.
II.	c^2.	.....	Grandchild.
	c^{1-1}.	Child of parent.	Brother or sister.
	c^{-1+1}.	Parent of child.	Consort.
	c^{-2}.	.....	Grandparent.
III.	c^3.	.....	Greatgrandchild.
	c^{2-1}.	Grandchild of parent.	Nephew or niece.
	c^{1-1+1}.	Child of parent of child.	Step-child.
	c^{1-2}.	Child of grandparent.	Uncle or aunt.
	c^{-1+2}.	Parent of grandchild.	Child-in-law.
	c^{-1+1-1}.	Parent of child of parent.	Step-parent.
	c^{-2+1}.	Grandparent of child.	Parent-in-law.
	c^{-3}.	.....	Greatgrandparent.
IV.	c^4.	.....	Greatgreatgrandchild.
	c^{3-1}.	Greatgrandchild of parent.	Grandnephew or grandniece.
	c^{2-1+1}.	Grandchild of parent of child.	Child of step-child.
	c^{2-2}.	Grandchild of grandparent.	First cousin.
	c^{1-1+2}.	Child of parent of grandchild.	Child of child-in-law.
	$c^{1-1+1-1}$.	Child of parent of child of parent.	Step-brother or step-sister.
	c^{1-2+1}.	Child of grandparent of child.	Brother-in-law or sister-in-law.
	c^{1-3}.	Child of greatgrandparent.	Granduncle or grandaunt.
	c^{-1+3}.	Parent of greatgrandchild.	Consort of grandchild.
	c^{-1+2-1}.	Parent of grandchild of parent.	Consort of brother or sister.
	$c^{-1+1-1+1}$	Parent of child of parent of child.	Consort of consort.
	c^{-1+1-2}.	Parent of child of grandparent.	Step-grandparent.
	c^{-2+2}.	Grandparent of grandchild.	Parent of child-in-law.
	c^{-2+1-1}.	Grandparent of child of parent.	Parent-in-law of parent.
	c^{-3+1}.	Greatgrandparent of child.	Grandparent of consort.
	c^{-4}.		Greatgreatgrandparent.

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When the expression for the relationship contains a change of sign in the index, it may in certain cases be equivalent to a relationship of a lower order; but when the index does not contain a change of sign, the relationship cannot be equivalent to one of a lower order. The third column contains the general meaning, and the fourth column the particular meaning when the relationship is supposed irreducible. The first reducible relationships are c^{1-1} and c^{-1+1}, which may reduce to 1. The relationship c^{2-1} may reduce to c; c^{2-2} may reduce to c^{1-1}, which we have seen may further reduce to 1. Consider the relationship c^{m-n}; the reducible forms are $c^{(m-1)-(n-1)}$, $c^{(m-2)-(n-2)}$, and so on, until one of the numbers is reduced to 0. Hence a relationship of an odd order can reduce only to one of an odd order, and a relationship of an even order only to one of an even order.

It is not difficult to conceive how this table of relationships (fully developed) may be useful to legislators in a case where an exact and comprehensive view of relationships is required, as, for example, in making a consistent and logical alteration of the laws of marriage. How many are the ways in which questions of comparative nearness of relationship arise, and how important to have a simple and ready means of settling them! If this table does not classify relationships according to their nearness, it at least provides the means for such a classification. The principles which have to be settled are—Supposing the distance of c to be measured as 1, what ought to be the value assigned to c^{1-1}, and what the value to c^{-1+1}? The former of these makes a relationship collateral, and the latter makes a relationship one of affinity. According to the method of reckoning degrees adopted by the Greek Church (which is very elaborate) c^{1-1} is reckoned 2, and c^{-1+1} is reckoned 0.

These general relationships are broken up into more specific relationships by the introduction of symbols to denote sex. Let a subscript m denote male, a subscript f denote female. Then

${}_mc$	denotes	son.	${}_mc^{-1}$	denotes	father.
${}_fc$	,,	daughter.	${}_fc^{-1}$	,,	mother.
c_m	,,	child of man.	c_m^{-1}	,,	parent of man.
c_f	,,	child of woman.	c_f^{-1}	,,	parent of woman.
${}_mc_m$	,,	son of man.	${}_mc_m^{-1}$	,,	father of man.
${}_mc_f$	,,	son of woman.	${}_mc_f^{-1}$	,,	father of woman.
${}_fc_m$	,,	daughter of man.	${}_fc_m^{-1}$	,,	mother of man.
${}_fc_f$	,,	daughter of woman.	${}_fc_f^{-1}$	,,	mother of woman

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Suppose we have a relationship of the second order, as *cc*. We may write :—

$$\Sigma ccU = \Sigma(m+f)ccU, \quad \ldots\ldots \quad (1)$$

$$= \Sigma(m+f)^2 ccU, \quad \ldots\ldots \quad (2)$$

$$= \Sigma(m+f)^3 ccU. \quad \ldots\ldots \quad (3)$$

In the case of (1), the terms in $m+f$ may be inserted before the first *c*, or between the two *c*'s, or after the second *c*. In the case of (2), the terms of $(m+f)^2$, which are *mm*, *mf*, *fm*, *ff*, may be inserted either in the first and second places, or in the first and third places, or in the second and third places. In the case of (3) the terms, which are *mmm*, *mmf*, *mfm*, &c., can be inserted in only one manner. Hence the number of forms which *cc* can take (counting *cc* itself as one) is 27.

To find the number of elementary relationships of the n*th Order.*

By an elementary relationship is meant a single relationship, in contrast to a relationship expressing the coexistence of several single relationships. The *n*th order has 2^n general relationships. Consider any one of these. A distinction of sex can be introduced before each *c* or c^{-1} and after the last. Hence the number of different ways in which a distinction of sex can be *r* times introduced is equal to the number of combinations of $n+1$ things *r* together. The number of different relationships obtained by the expansion of a term in which a distinction of sex has been *r* times introduced is 2^r. Hence the number of terms for one general notion is

$$1+(n+1)2+\frac{(n+1)n}{1.2}2^2+\ldots+2^{n+1};$$

that is, 3^{n+1}. Hence the total number for the *n*th order is $2^n 3^{n+1}$. For *n* being 5, the number is 23,328.

Cor. The number of elementary relationships included in the first *n* orders is $\frac{18}{5}(6^n-1)$; for *n* being 7, the number is greater than one million.

Laws of Reduction.—I. When the sex-symbols preceding and succeeding c^{l-1} are the same, then c^{l-1} can reduce to 1; and when they are different, it cannot reduce to 1. This depends on the morphological law, that sex in mankind is dicœcious.

II. When the sex-symbols preceding and succeeding c^{-l+1} are the same, then c^{-l+1} must reduce to 1; and when they are different, it cannot reduce to 1. This depends on physio-

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logical laws, in addition to the morphological law referred to above. Where polyandry prevails *must* becomes *can* in the case of *m*.

The expression *mm* between two relationship symbols is equivalent to *m*, and *ff* to *f*; but *mf* or *fm* imply a contradiction.

A compound relationship due to the coexistence of several elementary relationships, may be denoted by writing the relationships after one another, the order being immaterial—for example, a child of the man A, who is also a child of the woman B, by $c_m A c_f B$. When B is the same as A, a dot may be used to replace the first A; for example,

$$c_m c^{-1} A c_f c^{-1} A$$

(that is, a child of the father of A who is also a child of the mother of A) may be denoted by

$$c_m c^{-1} . c_f c^{-1} A.$$

It is convenient to have a special notation for a compound relationship which consists of a number of specific forms of one general relationship. This may be done by placing a vinculum over the general relationship, and by means of an index expressing the number of times the general relationship occurs. For example, $c^{\overline{1-1}^2}$ denotes full brother or full sister; $c^{\overline{1-1}^1}$ denotes half brother or half sister; and $c^{\overline{1-1}^0}$ is the appropriate expression for a non-brother or non-sister. According to the laws of this country we may have $c^{\overline{2-2}^4}$, $c^{\overline{2-2}^3}$, $c^{\overline{2-2}^2}$, $c^{\overline{2-2}^1}$, $c^{\overline{2-2}^0}$; that is, first cousin in four ways, first cousin in three ways, first cousin in two ways, first cousin in one way, and first cousin in no way. When distinctions of sex are introduced, the relationship of *first cousin* may have any one of 192 significations.

The notation for a compound term enables us to express the universal law that a person cannot be his or her own descendant. It is

$$1 . c^n A = 0$$

whoever A be, and *n* being any number from 1 upwards. The reciprocal aspect of the law is that

$$c^{-n} . 1A = 0;$$

that is, no person can be his or her own ancestor; while the most general statement of the law is

$$\Sigma c^m . c^{-n} A = 0,$$

provided *m* and *n* are not both 0.

In a similar manner it enables us to express the consequences

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of the laws of marriage of a given nation. For example,

$$\Sigma c_m \cdot c_f c^{s-1}_m \mathrm{A} = 0,$$

A being any British subject. This means that the children of any man A who are also children of any niece or daughter of the man A are none.

The principles of the English Law of Marriage are expressed as follows:—

I. For the direct line,

$$\Sigma c_m \cdot c_f (c^p + c^{-p})_m \mathrm{A} = 0,$$

where p may be any number. This means that the children of any man A must not be the children of any female descendant or ancestress of A.

II. For the collateral line,

$$\Sigma c_m \cdot c_f (c^{s-1} + c^{l-1} + c^{l-s})_m \mathrm{A} = 0.$$

This expresses that the children of any man A must not be the children of any niece, sister, or aunt of A.

III. For the direct line with one affinity,

$$\Sigma c_m \cdot c_f \{(c^p + c^{-p}) c^{-l+1} + c^{-l+1}(c^p + c^{-p})\}_m \mathrm{A} = 0.$$

This means that the children of any man A must not be the children of any female descendant or ancestress of any wife of A, nor the children of any wife of any descendant or ancestor of A.

IV. For the collateral line with one affinity,

$$\Sigma c_m \cdot c_f \{(c^{s-1} + c^{l-1} + c^{l-s}) c^{-l+1} + c^{-l+1}(c^{s-1} + c^{l-1} + c^{l-s})\}_m \mathrm{A} = 0.$$

This means that the children of any man A must not be the children of any niece, sister, or aunt of any wife of A, nor the children of any wife of any nephew, brother, or uncle of A.

As the result of my investigations, I am led to consider the Analysis of Relationships a special branch of the Algebra of Logic, that the processes of that method as described in my work 'Algebra of Logic' apply to this subject, and that we have special laws in addition, such as those mentioned above. In 1879 Dr. Halsted expressed his opinion* that little cosmos had not yet been brought out of the chaos of Relations; whether I have been successful in reducing a small part more approximately to order must be left to the reader of this article and of the original papers to judge.

I append some illustrations.

(1) *To express the relationship of Queen Victoria to William*

* Journ. of. Spec. Philos. vol. xiii. p. 107.

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the Conqueror. For our present purpose, the symbol s may be used to denote ${}_mc$ and d to denote c.

$$\text{Queen Victoria} = \dot{\imath}ds_4(s_1)^3 \text{ George I.};$$

that is, Queen Victoria is the only daughter of the fourth son of the first son of the first son of the first son of George the First.

$$\text{George I.} = s_1d_4\dot{\imath}d \text{ James I.}$$

$$\text{James I.} = \dot{\imath}s\left\{\begin{matrix}\dot{\imath}ds_1\\ s_1\dot{\imath}d\end{matrix}\right\} d_1 \text{ (Henry VII. + Elizabeth of York).}$$

$$\text{Henry VII.} = s\dot{\imath}ds^2s_3 \text{ Edward III.}$$

$$\text{Elizabeth of York} = d_1s_1s\left\{\begin{matrix}ss_4\\ ds_1\dot{\imath}ds_2\end{matrix}\right\} \text{ Edward III.}$$

$$\text{Edward III.} = (s_1)^4s_5s_1\dot{\imath}ds_4 \text{ William the Conqueror.}$$

$\therefore$ Queen Victoria =

$$\dot{\imath}ds_4s_1^4d_4\dot{\imath}d\dot{\imath}s\left\{\begin{matrix}\dot{\imath}ds_1\\ s_1\dot{\imath}d\end{matrix}\right\} d_1\left\{\begin{matrix}s\dot{\imath}ds^2s_3\\ d_1s_1s\left\{\begin{matrix}ss_4\\ ds_1\dot{\imath}ds_2\end{matrix}\right\}\end{matrix}\right\} s_1^4s_5s_1\dot{\imath}ds_4 \text{ William the Conqueror.}$$

If we wish to express the general nature of this relationship, we have

$$\text{Queen Victoria} = \overline{c^{25}}^4 . \overline{c^{27}}^2 \text{ William I.};$$

that is, Queen Victoria is a descendant of the fourth degree and twenty-fifth order, and also a descendant of the second degree and twenty-seventh order of William the First. This example indicates that the notation of this analysis may prove useful in condensing and rendering more exact the information supplied in *Peerages*.

(2) Given that Stephen was a grandson of William I., and Henry II. a greatgrandson of William I., what follows as to the relationship of Henry to Stephen?

$$S = {}_mc^2{}_mW, \quad H = {}_mc^3{}_mW,$$
$$\therefore H = {}_mc^3{}_mc^{-2}{}_mS;$$

which can reduce to $c^{2-1}S$ and cS. Hence Henry was either a son of a first cousin of Stephen, or a nephew of Stephen, or a son of Stephen.

(3) "Sister or brother have I none;
But that man's father was my father's son."

Let A denote the speaker and X the person referred to.

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Then the first line gives us the equation

$$\Sigma cc^{-1}A=2A; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the second line gives the equation

$$_mc^{-1}{}_mX={}_mc_mc^{-1}A. \quad . \quad . \quad . \quad . \quad (2)$$

From (1) it follows that

$$\Sigma c_mc^{-1}A=A;$$

therefore substituting in (2), we get

$$_mc^{-1}{}_mX=A;$$

that is, A was the father of the man X.

The Rule for transposing a relationship from one side of an equation to the other is as follows:—*Change the order of the symbols, and change each symbol into its reciprocal, the reciprocal of* m *being* m, *and of* f *being* f. Applying this rule to the above, we get

$$X={}_mc_mA;$$

that is, X was a son of the man A.

(4) A lady, on being asked about a photograph in her album, replied:—You know that I have no daughters; well, that person's daughter's son was the father of a grandchild of mine.

Let A denote the lady and X the person. Then

$$\Sigma_fc_fA=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$_mc_fcX={}_mc^{-1}cc_fA. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From (1),

$$\Sigma c_fA=\Sigma_mc_fA;$$

and from (2), by the above rule of transposition,

$$X=c^{-1}{}_fc^{-1}{}_mc^{-1}cc_fA;$$

therefore from (1),

$$X=c^{-1}{}_fc^{-1}{}_mc^{-1}c_mc_fA.$$

But $_mc^{-1}c_m=1$ always (p. 440); therefore

$$X=c^{-1}{}_fc^{-1}{}_mc_fA.$$

Also $_fc^{-1}c_f=1$ always; therefore

$$X=c^{-1}{}_fA;$$

that is, X was either the father or the mother of the lady. Thus the answer given to the question was perfectly determinate.

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(5) Deduce all the logical consequences of the law which prohibits a man marrying a sister of his deceased wife.

The primary equation is

$$\Sigma 1 \,.\, {}_{m}c^{-1}c_{f}cc^{-1}{}_{f}c^{-1}c_{m}\mathrm{A}=0,$$

where A may be any inhabitant of the British Islands. It expresses that a man cannot (lawfully) be the husband of a sister of his wife. The Rule for transforming an equation which is true for any A is as follows:—

To transform a universal equation which has a compound term of the second degree equated to 0, *suppose all the symbols brought to one factor in accordance with the rule of transposition given above* (p. 444); *then removing a symbol from the front gives one derived equation, and removing a symbol from the end gives another derived equation. Transform each of these two in a similar manner, then each of their four resultants, and so on until all the terms have been brought to the other factor. The total of these derived equations is the total number of transformations of the given universal equation.*

Applied to the above equation, transposition of the first symbol gives

$$\Sigma c_{m} \,.\, c_{f}cc^{-1}{}_{f}c^{-1}c_{m}\mathrm{A}=0;$$

that is, a child of a man cannot be the child of a sister of a wife of the man. Transposition of the last symbol gives

$$\Sigma_{m}c^{-1} \,.\, {}_{m}c^{-1}c_{f}cc^{-1}{}_{f}c^{-1}\mathrm{A}=0.$$

that is, the father of a person cannot be the husband of a sister of the mother of that person. By continuing the process other 19 forms (different from one another) are obtained, which respectively express that—

A wife of a man cannot be the sister of a wife of the man.
A child of the father of a person cannot be the child of a sister of the mother of the person.
A husband of a woman cannot be the husband of a sister of the woman.
A parent of a wife of a man cannot be the parent of another wife of the man.
A step-mother of a person cannot be the sister of the mother of the person.
A step-child of a woman cannot be the child of a sister of the woman.
A son-in-law of a person cannot be the husband of another daughter of the person.
A parent of a step-mother of a person cannot be the parent of the mother of the person.
A wife of a husband of a woman cannot be the sister of the woman.
A child of a husband of a daughter of a person cannot be the child of another daughter of the person.
A sister of a step-mother of a person cannot be the mother of the person.
A parent of another wife of a husband of a woman cannot be the parent of the woman.

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Another wife of a son-in-law of a person cannot be the daughter of the person.
A step-child of a sister of a woman cannot be the child of the woman.
A child of a sister of a step-mother of a person cannot be the person.
A sister of a wife of a husband of a woman cannot be the woman.
A parent of another wife of a son-in-law of a person cannot be the person.
A wife of a husband of a sister of a woman cannot be the woman.
A child of a husband of a sister of the mother of a person cannot be the person.

LIX. *On an Electrochemical Method of Investigating the Field of Electrolytic Action.* By Alfred Tribe *.

THE electrochemical method of investigating the field of electrolytic action has for its basis new facts, the nature of which I propose to set forth in the first part of this communication, reserving the second part for the description and discussion of the results which have recently accrued from its application.

The Method.

When a rectangular plate of metal unconnected with the battery is placed lengthwise in an electrolyte undergoing electrolysis, the plate does work identical in kind with that being done by both electrodes. The electropositive ion of the electrolyte separates and distributes itself on a portion of the plate nearer the + electrode, and the electronegative ion on another part of the plate nearer the — electrode. The respective boundaries of these ions are sharply defined, and the intermedial space free from either ion.

When an aqueous solution of copper sulphate is electrolyzed with silver electrodes, copper of course separates on the — electrode; but more or less of the dark-grey or black silver peroxide forms on the + electrode. $\frac{1}{100}$ of a weber in one minute produces, in fact, a sensible separation of copper and a sensible formation of silver peroxide on a silver plate 34 millim. × 7 millim.

It follows, therefore, that a silver plate placed in a solution of copper sulphate, under the conditions named in the first paragraph, should have copper deposited on that part which may be supposed to receive — electrification, and silver peroxide on that which receives the positive. Such is the case.

The registration of any set of electrifications in this way by the ions of electrolytes need take only a few minutes, the minimum time being determined by the dimensions of the analyzer †, strength of electrolyte, and available current.

* Communicated by the Author, having been read at the Meeting of the French Association, Algiers, April 1881.

† The rectangular silver plate is named, for convenience, the *analyzing plate*, or, in brief, the *analyzer*.

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After the electric energy has done enough work on the analyzer for the action to be visible, time is seen to exert no influence in determining the magnitudes of the distributions. Thus in 6 minutes and 60 minutes respectively, identical distributions were recorded by similar analyzers, other circumstances being the same. But the magnitudes of the distributions registered by an analyzer of given dimensions vary with every condition which may be supposed to alter the chemical or physical state of the electrolytic medium, as temperature, proportion of water, and electric quantity.

When at ordinary temperatures the analyzer is placed with its length perpendicular to the electrodes in a homogeneous field*, although the superficial magnitudes of the ions vary, as stated, with any variation in the condition of the electrolytic medium, yet in every case one quality is seen to obtain, namely a similarity of the same ion on the two sides of the analyzer, both as regards magnitude and configuration of its boundary-line. This happens whether the plate has its shorter edge vertical or is supported with its sides horizontal. Under these circumstances the boundaries of the ions are practically parallel with the shorter edge. Distributions having all these characteristics are named *parallel* (figs. 1 and 3).

When the course of the energy makes an oblique angle with the edges of the analyzer, but remains parallel to the sides of the latter, the electrifications recorded are also the same on the two sides of the plate; but the boundary-lines of the ions now cross the plate obliquely to its shorter edge (figs. 4, 5, and 6). The positive ion on the longer edge in opposition to the course of the energy is greatest in length, while the negative ion on the same edge of the plate is smallest in length. The obliquity and the intermedial space between the ions increase as the longer edge of the analyzer approaches a line at right angles to the direction of the influence. But at whatever angle the plate is fixed, the boundary-lines of the ions are parallel with the electrodes, and therefore at right angles to the direction in which the energy is transmitted.

When the electric power makes an oblique angle with the *sides* of the analyzer, the electrifications recorded present a totally different character. The magnitudes of the same ion and the configuration of its boundary-line are now very different on the two sides of the plate. On the side in opposition to the direct course of the energy, the configuration of the positive ion is markedly convex and greater in magnitude than it is on the reverse side of the plate, where, more-

* That is, where the electrodes are of the same depth and breadth as the cross section of the electrolyte.

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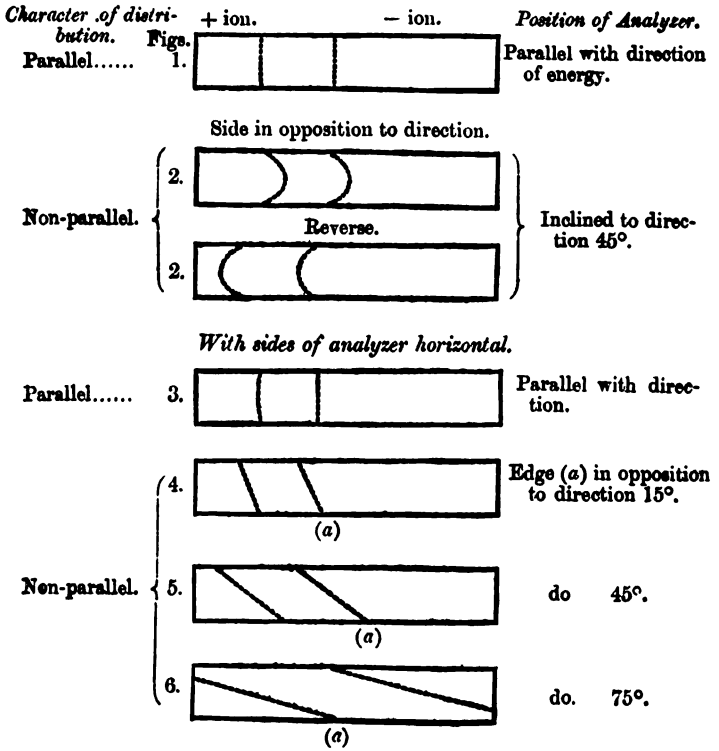
over, the configuration of this same ion is markedly concave (figs. 2, 2). Further, the configuration of the boundary-line of the negative ion on the first-named side of the analyzer is concave and smaller in magnitude than on its reverse side, where again the boundary-line of the ion is convex. The intermedial space between the ions increases as the analyzer approaches a position at right angles to the right line between the electrodes, and the characteristic boundary configurations become more and more marked. At right angles the positive ion is smaller in quantity, and arranged longitudinally along the centre of the side of the analyzer facing the + electrode, while the negative ion on the reversed side is similarly disposed, but generally not so well defined.

The classes of distribution described in these two last paragraphs are named *non-parallel*. The dotted lines in the annexed diagram exhibit the boundary-lines and general characteristics of the parallel and non-parallel distributions.

With shorter edge of analyzer vertical.



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It is obvious that the results described furnish the data for determining with ease and precision, first, physical differences in parts of an electrolytic field; and, secondly, the direction in which the energy is being transmitted relatively to either side or edge of the analyzing-plate, and therefore of its direction in the electrolytic medium.

Results.

I. *Demonstration of Differences in corresponding parts of Non-homogeneous Fields.*—Few phenomena are more generally known than those of the electric discharge. The feature common to these phenomena is a well-marked difference between the spaces in the immediate vicinity of the + and — electrodes. No differences, as far as I am aware, have hitherto been shown to obtain in corresponding parts of electrolytic media. But the supposition that dielectrics and electrolytes differ in degree rather than in kind, as regards the mode in which they transmit the electric influence, is founded on the analogy in the results obtainable in the two media, and is supported by the continuity in electrical qualities evidently prevalent among substances in general. Differences in corresponding parts in the vicinity of the + and — electrodes in certain electrolytic fields might not unreasonably therefore be expected to exist; and the question naturally arose whether the method of research, the principles of which have been set forth, would reveal such differences.

In a homogeneous field, analyzers* placed perpendicular to the electrodes in any part of the field showed that they were surrounded by the same conditions; that is, the magnitudes of the ions on an analyzing plate were identical close to the — electrode, close to the + electrode, and midway between the electrodes. But on passing to non-homogeneous fields (*i. e.* where the electrodes are smaller than the transverse section of the electrolyte) this was no longer the case. The magnitudes of the ions on exactly similar analyzers placed perpendicular, in the centre of, and close to these electrodes, in exactly corresponding positions, were markedly different, and the difference increased as the width of the electrodes relative to the cross section of the electrolyte was reduced.

Differences in corresponding parts of a non-homogeneous field in the vicinity of the electrodes of opposite name are demonstrable, then, by this method of working, and also that the differences are in some way connected with the power of

* In all the experiments to be described, the analyzer was placed with its shorter edge vertical in the electrolytic medium.

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extension of the influence, as no difference whatever is detectable in corresponding parts of a homogeneous field.

II. *Demonstration of the Direction in Electrolytic Fields.*—It follows from the generally received views of electric conduction that, in every part of a homogeneous electrolytic field, the energy is transmitted perpendicularly to the electrodes. The electrochemical method shows, at least, that the direction in this case is everywhere identical. Also it follows that, in certain parts of non-homogeneous fields, the direction is curvilinear. A direct experimental proof of differences in direction in a non-homogeneous field was furnished by the analysis of one of considerable dimensions. In a line joining the centre of the electrodes in this experiment and in another at right angles at its centre, *parallel* distributions were recorded by analyzers placed lengthwise perpendicular to those sides of the cell where the electrodes were situated. Analyzers placed, however, in the same direction, but a few millimetres from the sides of the cell, and at various distances from the electrodes, recorded *non-parallel* distributions. It is demonstrable, then, that, in the positions just named, the direction of transmission of the influence forms an angle more or less acute with the sides of the analyzer. Whether the direction results from emission of the influence at various angles from the plane surface of the electrodes, or leaves them always at right angles, and is subsequently driven out of its direct course by the resistance of the medium, remain to be determined.

It may be asked whether the facts just set forth could have been discovered by any other method than the one now described. No other method of which I am aware is capable of demonstrating so directly the direction of the influence in the several parts of a non-homogeneous field. Whether the method employed by De La Rive in 1825, or the one used a few years ago by Prof. Adams, for investigating the laws of electric distribution in electrolytes, is capable of exhibiting all physical differences in the corresponding parts of non-homogeneous fields cannot even be conjectured, until the relation between the magnitudes and the quantities of the ions on the analyzer in these parts of the field has been studied.

Whatever applications of this graphic method may be found, it possesses the advantages of great simplicity, and of the results being self-recorded and permanent. It may be expected from its nature to assist in the demonstration and elucidation of the laws of transmission of electricity through electrolytic media, and in revealing, it is hoped, the inner nature of the electrolytic process itself.

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A detailed account of the results obtained by this method, including determinations of the electric distribution on metallic conductors, will be found in a paper recently communicated by me to the Royal Society.

Dulwich College, April 1881.

LX. *On the Law of Force between Electric Currents.* *By* H. W. Watson *and* S. H. Burbury*.

1. THE laws of mutual action between electric currents, or between separate elements of electric currents, have been investigated by Ampère, and, following him, by F. E. Neumann, Weber, Helmholtz, Clausius, and others. Their object has been to discover a law of force between the elements which should give results in accordance with facts established, or supposed to have been established, by experiment. Ampère's experimental data are given by Professor Tait in his work on Quaternions, second edition, p. 250, as follows, using his own words :—

"I. Equal and opposite currents in the same conductor produce equal and opposite effects on other conductors; whence it follows that an element of one current has no effect on an element of another which lies in the plane bisecting the former at right angles.

"II. The effect of a conductor bent or twisted in any manner is equivalent to that of a straight one, provided that the two are traversed by equal currents, and the former nearly coincides with the latter.

"III. No closed circuit can set in motion an element of a circular conductor about an axis through the centre of the circle and perpendicular to its plane.

"IV. In similar systems traversed by equal currents the forces are equal."

To these canons, deduced from experiments of Ampère, may now be added :—

V. Oersted's experiments, showing that the action of a closed circuit is equivalent to that of a magnetic shell of proper strength whose boundary coincides with that of the circuit.

Also VI., an experiment of which the details are given in Maxwell's 'Electricity,' vol. ii. p. 149, which is supposed to prove directly that the force exerted by any closed electric circuit on any element of another circuit is always normal to the element. The law of force, whatever it be, must satisfy these conditions.

* Communicated by the Authors.

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2. On these canons we make the following observations.

With regard to I., it will be observed that it consists, first, of a statement of fact—namely, that a reversal of one current reverses the effect; secondly, of an inference supposed to follow from the observed fact—namely, that an element of one current has NO effect on an element of another which lies in the plane bisecting the former at right angles. Let the first element be at the origin in direction x, and the second anywhere in the plane of y, z. If, when the first element is in the positive direction, the second is attracted, it follows, by the general principle enunciated, that were the first element in the negative direction, the second would be repelled. Now we cannot imagine any reason why in the former case the force should be an attraction and in the latter a repulsion, any more than the converse. Hence it is concluded, no doubt rightly, that there can be no *force* tending to move the second element in the plane of y, z.

But it should here be observed that the reasoning would not apply to a couple tending to turn the second element round an axis without changing the position of its centre. The hypothesis, for instance, that the first element tends to turn the second into a position parallel to the first, agrees with the general law enunciated in I., and is not open to *à priori* objection.

II. is equivalent to the principle, which is universally assumed in all treatises on the subject, that any elementary current may be replaced by its components, the middle points of the components being identical with that of the element.

IV. leads to the conclusion, as shown in Maxwell's 'Electricity,' vol. ii., that the forces of attraction between two elements are inversely proportional to the square of the distance between them.

V. It follows from Oersted's experiments, that the mutual action of two closed electric circuits is the same as that of two magnetic shells bounded by the circuits. It must therefore have a potential; and such potential must be of the form

$$\mu i i' \iint \frac{\cos \epsilon}{r} ds\, ds'^{*},$$

in which i, i' are the strengths of the currents, and ϵ is the angle, and r the distance between an element ds of the one,

* We take the positive sign in the same way as it is usual to say the potential of unit mass of matter at distance r is $\frac{1}{r}$.

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and an element ds' of the other circuit, and μ is a constant depending on the nature of the medium in which the shells are placed.

3. If we were here to assume that the potential of mutual action between *each pair of elementary currents* is of the same form,

$$V = \mu i i' \frac{\cos \epsilon}{r} ds\, ds',$$

we should of course obtain the correct value for the potential of two closed circuits; and therefore this assumption would satisfy all the results obtained from experiments with closed circuits. This form of mutual potential was proposed by F. E. Neumann, but seems to have been abandoned, because it would not satisfy the results obtained, or supposed to have been obtained, in experiments with open currents. Now, according to the views of Maxwell, no such thing can exist within the range of our experiments as an unclosed current, because the current, if not closed by conductors, closes itself by means of change of displacement in the dielectric. If this be true, no experiments can ever lead to results inconsistent with the above simple law of potential. Further, even without assuming the truth of Maxwell's theory, it appears to us that the experiments which were supposed to be inconsistent with the above law admit of interpretation consistent with it, as we hope to show.

4. It was believed, however, to be established by experiment, that the attractive force exerted by any closed circuit upon any element of another current is always normal to the element (canons III. and VI.). According to the above law of F. E. Neumann, the impressed force exerted by the closed circuit on an isolated element would not necessarily be normal to it. It was thought necessary, therefore, to invent a law of force between two elementary currents which should satisfy this supposed experimental result, and at the same time should give the correct value for the potential of two closed circuits.

According to F. E. Neumann's law, the force between two elements of given strength depends only on their distance and the angle, ϵ, which their directions make with one another; it is independent of the angles θ and θ' which their directions make with r, the line joining them. But, by a known geometrical theorem,

$$\iint \frac{\cos \epsilon}{r} ds\, ds' = \iint \frac{\cos \theta \cos \theta'}{r} ds\, ds'$$

for any two closed curves in space. Hence, if we assume for

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the potential of two elementary currents ids and $i'ds'$ the form

$$ii'\left\{A\frac{\cos\epsilon}{r}+B\frac{\cos\theta\cos\theta'}{r}\right\}ds\,ds',$$

where A and B are any constants, we should for any pair of closed circuits obtain as the potential an expression proportional to

$$ii'\iint\frac{\cos\epsilon}{r}ds\,ds',$$—

that is, practically identical with that obtained from F. E. Neumann's law. But the force exerted by a closed circuit on a *separate element* of another would not be identical in the two cases; and it may by proper choice of the constants A and B be made always normal to the element.

5. Ampère's solution of the problem is as follows, assuming a force and not a potential between two elementary currents. Let $i\cos\theta\,ds$, $i'\cos\theta'\,ds'$, according to the usual notation, be the component parts of the elements resolved in r, the line joining their centres; and $i\sin\theta\,ds$, $i'\sin\theta'\cos\phi\,ds'$, the component parts perpendicular to r in the plane of r and ds. Then it is assumed that the two radial components attract each other with a force in the direction of r varying inversely as the square of the distance, viz.

$$\frac{a}{r^2}\cos\theta\cos\theta'ii'\,ds\,ds',$$

and the two transverse components attract each other with a force in the direction of r, viz.

$$\frac{b}{r^2}\sin\theta\sin\theta'\cos\phi ii'\,ds\,ds',$$

where a and b are constants. It is then shown that if the relation between a and b be $2a+b=0$, the desired result will follow —namely, that the force exerted by any closed circuit on any element of a current *is* always normal to the element. This relation, then, satisfies Professor Tait's canon III. above given, and also satisfies VI. It will be found also to lead to the correct expression for the mutual potential of two closed circuits, and therefore satisfies all experimental conditions.

Ampère's results may be concisely expressed as follows; viz. the action of $i\,ds$ upon $i'\,ds'$ is a force in the direction of the line, r, which joins them, and whose intensity is

$$\frac{ii'}{\sqrt{r}}\cdot\frac{d^2\sqrt{r}}{ds\,ds'}ds\,ds'.$$

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This law of force gives no potential of the action of one element upon another; but, as we have said, it gives the potential

$$ii' \iint \frac{\cos e}{r} \, ds \, ds'$$

for the mutual action of two CLOSED circuits.

6. Weber has further shown that if an electric current be supposed to consist of positive electricity moving with velocity v in one direction, and an equal quantity of negative electricity moving with the same velocity in the opposite direction, then a certain hypothesis regarding the force exerted on one another by particles of electricity in motion not only leads to Ampère's law of force, but also explains the ordinary phenomena of induction by variation of the primary current, or by variation of the position of the circuits. Weber's hypothesis is that the mutual potential of two particles of electricity e and e' is

$$\frac{ee'}{r}\left\{1 - \frac{1}{2c^2}\left(\frac{dr}{dt}\right)^2\right\},$$

where c is a constant. If the particles be at rest, $\frac{dr}{dt} = 0$, and this expression gives the ordinary electrostatical potential $\frac{ee'}{r}$. If they be in motion, it will be found to lead to Ampère's law, as shown by Briot*. The coincidence appears at first sight remarkable, and has done much to facilitate the acceptance of Ampère's results as well as Weber's. It will be seen, however, on further considering the subject, that Ampère and Weber both start from the same fundamental assumption with regard to the nature of the action between two elementary currents—viz. that it depends not only on their directions relative to each other, but also on their directions relative to the line joining them. This may perhaps account for their leading to the same result.

7. Stefan has shown† that, assuming the only forces acting to be, as Ampère assumes,

$$\frac{a}{r^2} \cos\theta \cos\theta' ii' \, ds \, ds',$$

and

$$\frac{b}{r^2} \sin\theta \sin\theta' \cos\phi ii' \, ds \, ds',$$

then, if there be a potential for two closed circuits, such poten-

* *Théorie mécanique de la Chaleur*, chap. ix.
† *Sitzungsberichte*, Vienna, 1869.

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tial can have no other form than

$$V=\mu ii'\iint\frac{\cos e}{r}\,ds\,ds',$$

and, further, that there can be no potential at all unless the relation between the constants a and b be $2a+b=0$, which was the relation obtained by Ampère from different reasoning. It thus appears again that Ampère's law leads to the correct expression for the potential.

Stefan has, further, considered the consequences of assuming mutual action in the nature of attraction or repulsion between the radial component of one, and the transverse component of the other element. The radial component of ds is $i\cos\theta\,ds$; the transverse component of ds' in plane of r and ds is $i'\sin\theta\cos\phi\,ds'$. If we suppose $i\cos\theta\,ds$ to exert on $i\sin\theta\cos\phi\,ds'$ a force tending to move it in the direction of r,

$$ii'\frac{c}{r^2}\cos\theta'\sin\theta'\cos\phi\,ds\,ds',$$

and, in like manner, the transverse component of ds, namely $i\sin\theta\,ds$, to exert on $i'\cos\theta'\,ds'$, the radial component of ds', a force in direction of r,

$$ii'\frac{d}{r^2}\sin\theta\cos\theta'\,ds\,ds',$$

where c and d are two new constants, then Stefan shows that the potential, if there be a potential, must have the same form as before, and that there can be no potential unless

$$2a+b+c-2d=0,$$

which, by making $c=0$ and $d=0$, includes Ampère's law as a particular case.

8. Carl Neumann, in a very elaborate memoir*, has deduced the following as the attractive force between the two elementary currents $i\,ds$, $i'\,ds'$, viz.

$$-ii'\left\{\frac{2}{r^2}\cos e-\frac{1}{r^2}\cos\theta\cos\theta'\right\}ds\,ds',$$

and this without assuming that the closed circuit exerts a normal force on each element of another current.

C. Neumann, however, excludes from his consideration "couple-action," whereby one element may tend to turn another round an axis without altering its position in space.

9. We may here add that Clausius considers that the x

* *Ueber die den Kräften electrodynamischen Ursprungs zuzuschreibenden Elementargesetze*: Leipzig, 1873.

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component of the force which a moving particle of electricity e exerts on another, e', is expressed by the formula

$$ee'\left\{\frac{d\frac{1}{r}}{dx}\left(-1+k\left(\frac{dx}{dt}\frac{dx'}{dt}+\frac{dy}{dt}\frac{dy'}{dt}+\frac{dz}{dt}\frac{dz'}{dt}\right)\right)-k\frac{d}{dt}\left(\frac{1}{r}\frac{dx}{dt}\right)\right\},$$

where x, y, z relate to the first and x', y', z' to the second particle (see Phil. Mag. October 1880).

10. It seems that the Continental physicists consider the force between two elementary currents as a force of translation only, excluding from their consideration "couple-action," whereby one element tends to turn the other round an axis without altering its position in space. But such couple-action results immediately, as will be shown later, from F. E. Neumann's law of potential.

11. Maxwell has treated the subject in a different way. While giving Ampère's reasoning and his results, as well as those of Weber, he founds his own theory on the hypothesis that in nature all circuits are closed circuits, and proceeds to obtain the known results of experiment, by the application of ordinary dynamical methods to the theory of closed circuits—that is, that if the circuits be not closed by conductors, there arise changes of displacement of electricity in dielectrics which have the effect of closing the currents.

12. Now, if it be true, as Maxwell teaches, that in nature, or within the range of our experiments, all currents are closed, there can be no necessity for devising elaborate laws of force. We have only to assume for each pair of elements the potential

$$V=\mu ii'\frac{\cos\epsilon}{r}\,ds\,ds',$$

and we cannot fail to obtain results in accordance with experiment, and, in fact, exactly the same results as are obtained from any of the more complicated laws above stated; so that, on this view, no experiment could furnish any reason for accepting one law rather than the other. It appears to us, then, that if Maxwell's theory, that all currents are closed, be accepted, the law that, for each pair of elementary currents,

$$V=\mu ii'\frac{\cos\epsilon}{r}\,ds\,ds'$$

ought to find provisional acceptance also.

13. But as Maxwell's theory is not accepted universally, we proceed to examine some of the experimental evidence on which the doctrine is based, that a closed circuit exerts a

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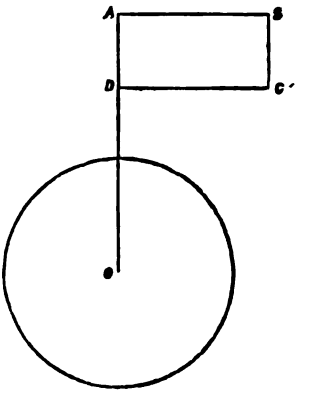
normal force only on every elementary current (canons III. and VI.), this being the only experimental fact inconsistent with the simple law.

The experiments quoted in support of this are the following:—

(1) A current in a circular conductor, movable about an axis through its centre perpendicular to its plane, remains unmoved in the field of another closed circuit.



(2) A current in a rectangular conductor A B C D, movable about one side A D, which coincides with a line through the centre, O, of a circular conductor, the line A D being perpendicular to the plane of the latter, remains immovable when a current passes through the circular conductor.

(3) (Maxwell, vol. ii. p. 149, cited above.) A circular conductor, C D, movable about its centre in its own plane, passes through two mercury-cups fixed at A and B, at which a current enters and leaves the conductor respectively; and no motion ensues when a closed current is brought into the neighbourhood.

Now, in point of fact, all these three experiments would be equally satisfied by any law which gave a potential between two currents. For in (1) and (2) no possible displacement of the movable circuit could alter that potential, if it existed, as is obvious from the symmetry of the arrangements in the original and displaced positions respectively. Since, therefore, no displacement could alter the potentials if they existed, it follows that if there be a potential there would be no force producing motion.

The same thing is true of No. 3, although at first sight not so evident; for since the current enters and leaves the conductor at the points A and B fixed in space, and the arc A B is fixed in space, it follows that, notwithstanding any motion of the conductor, the current remains fixed in space, and the potential must remain the same whether the conductor be moved or not. Hence there can be no tendency to move the conductor.

These three experiments, therefore, do not prove that the

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law of action between current-elements is necessarily such that the resultant action of a closed circuit on an element is normal to that element; they would be equally satisfied by *any* law of action giving a potential between two closed circuits.

14. Indeed it may be shown directly that, whether there be any tangential action or no, it never can be manifested by experimenting upon a closed circuit; and therefore, if all circuits be necessarily closed circuits, it follows that such tangential action never can be manifested.

For such closed circuit must be infinitely flexible and infinitely extensible (at least in the neighbourhood of the element acted on), otherwise the element will be influenced by the mechanical action of the adjacent parts of its own circuit; and if it be perfectly flexible and extensible, no virtual motion of any element in the direction of its length can alter the value of the potential

$$\iint \frac{\cos \epsilon}{r} ds\, ds'.$$

15. Again, it is generally stated to be a deduction from Ampère's law, that the action of a solenoid of currents of infinitely small section and indefinitely extended in one direction, upon an element of a current, is a force perpendicular to the plane passing through the element and the extremity of the solenoid, varying inversely as the square of the distance of the element from that extremity, and directly as the sine of the angle between the element and that distance. Ampère's law would give exactly this result, while that now proposed would not do so, for an element considered alone, *i. e.* otherwise than as part of a closed or infinite current. It is clear, for the reasons mentioned above, that no experiment can give a result in this form. What experiment really proves is, that in the case of a closed circuit or infinitely extended rectilinear current, the action on the solenoid is the same as if that of each element of the current followed the above law. And the law of F. E. Neumann leads exactly to the same conclusion. For let the infinite current be of strength i in the axis of z, z; then it is evident by integration that Ampère's law of elementary current action would lead to a potential energy between the whole current and the solenoid of the form $i \tan^{-1} \frac{y}{x}$, or, more generally, when the solenoid is not infinite, to the form

$$i \left\{ \tan^{-1} \frac{y}{x} - \tan^{-1} \frac{b}{a} \right\}.$$

For according to the law now proposed, the potential between

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the current-element $i\delta z$ and one of the solenoid currents, whose strength is i', would be

$$ii'\delta z \int \frac{\cos\epsilon}{r}\, ds',$$

the integration being taken round the elementary circuit. By Stokes's theorem this becomes

$$\frac{ii'\mathrm{A}(ly-mx)}{r^3},$$

where A is the area of the solenoid-section, l, m, n the direction-cosines of its normal, and r the distance of its centre from δz. Therefore the potential energy between the whole infinite current and this section of the solenoid is V, where

$$\mathrm{V} = \mathrm{A}ii' \int_{-\infty}^{\alpha} \frac{ly-mx}{(x^2+y^2+z^2)^{\frac{3}{2}}}\, dz$$

$$= \mathrm{A}ii' \frac{ly-mx}{x^2+y^2}.$$

And the potential of the whole solenoid is

$$\mathrm{A}ii' \int \frac{y\frac{dx}{ds} - x\frac{dy}{ds}}{x^2+y^2}.\, dz,$$

or

$$\mathrm{A}ii' \left\{ \tan^{-1}\frac{y}{x} - \tan^{-1}\frac{b}{a} \right\}.$$

16. Again, it is generally stated that the action of a particle of imaginary magnetic matter upon a current-element varies as the sine of the angle between the distance of the particle from the element and the element directly, and as the square of that distance inversely, and is perpendicular to the plane passing through the particle and the element. For the reasons mentioned in the case of the solenoid, it is obvious that no such law of action as this can be experimentally proved to exist between a current-element and a magnetic pole; but the equivalent law of action between an infinite current and a particle of imaginary magnetic matter may be deduced from the law of current-action now proposed, combined with the experimental fact that the potential of a current-circuit and a magnetic shell is equivalent to that between the circuit and the current bounding the shell of suitable intensity. For if i be the intensity of a current in the axis of z, and i' that in the bounding circuit of a plane magnetic shell of very small area A, it will follow, as in the case last considered, that the poten-

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tial between the shell and current is

$$\mathrm{A}ii'\frac{ly-mx}{x^2+y^2},$$

the symbols having the same meaning as before.

If, now, the potential of positive magnetic matter on one face of the area A relative to the infinite current be $\mu\mathrm{A}i\phi(x,y)$, that of the negative matter on the opposite face will be

$$-\mu\mathrm{A}i\phi(x,y)-\mu\mathrm{A}i\left\{\frac{d\phi}{dx}l+\frac{d\phi}{dy}m\right\}h,$$

where h is the thickness of the shell; and the resulting potential is

$$-\mu\mathrm{A}i\left\{\frac{d\phi}{dx}l+\frac{d\phi}{dy}m\right\}h.$$

But this has been shown to be equal to

$$\mathrm{A}ii'\frac{ly-mx}{x^2+y^2}.$$

Therefore these expressions must be equal for all values of l and m;

$$\therefore \mu h\frac{d\phi}{dx}=-\frac{i'y}{x^2+y^2},\quad \mu h\frac{d\phi}{dy}=+\frac{ix}{x^2+y^2};$$

$$\therefore \phi(x,y)=\frac{i'}{\mu h}\tan^{-1}\frac{y}{x},$$

and

$$\mu\mathrm{A}i\phi(x,y)=\frac{\mathrm{A}ii'}{h}\tan^{-1}\frac{y}{x},$$

giving the law required.

We have here shown in detail that these two results are deducible from the law of F. E. Neumann; but it is evident that we might have inferred this without further proof from the fact that we are throughout dealing with complete circuits, for which it has been already shown that the mutual potential is in all cases the same as that given by Ampère's law.

17. The adoption of the above law for the potential of two elementary currents, viz.

$$\mathrm{V}=\mu ii'\frac{\cos\epsilon}{r}\,ds\,ds',$$

agrees perfectly with Maxwell's treatment of the subject, although Maxwell nowhere distinctly states the existence of the law, perhaps on account of its conflict with canon III. mentioned at the beginning of this paper, the validity of which

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canon we have been calling in question. Moreover this law (if not disproved by some decisive experiment) would greatly simplify the mathematical treatment of electrodynamics.

Since for two elements

$$V = \mu i i' \frac{\cos \epsilon}{r} ds\, ds',$$

we have

$$\frac{dV}{dr} = -\mu i i' \frac{\cos \epsilon}{r^2} ds\, ds'.$$

For parallel currents in the same direction $\cos \epsilon = 1$; in opposite directions, $\cos \epsilon = -1$. Therefore, "*Parallel elementary currents, if in the same direction, attract each other with a force varying inversely as the square of the distance; if in opposite directions, repel each other with the same force.*"

For perpendicular currents, $\cos \epsilon = 0$; therefore $\frac{dV}{dr} = 0$, or "*Elementary currents at right angles to each other exert on each other no attractive or repulsive force.*"

Again,

$$\frac{dV}{d\epsilon} = -\mu i i' \frac{\sin \epsilon}{r} ds\, ds'.$$

For parallel currents, $\sin \epsilon = 0$; for perpendicular currents, $\sin \epsilon = 1$. Therefore "*Parallel elementary currents exert on each other no couple-action.*"

"*Elementary currents at right angles to each other tend to turn each other into a position of parallelism with a force varying inversely as the distance.*"

The plane in which either element tends to turn is that in which the variation of $\cos \epsilon$ for a given small angle turned through is a maximum. Resolving the elements into their components, we find that the component $i\,dx$ tends to turn $i'\,dy'$ round the axis of z, and $i'\,dz'$ round the axis of y; hence the turning-couple exerted by $i\,ds$ on $i'\,ds'$ is

$$\mu i i' \left\{ \frac{dx}{ds} \frac{dy'}{ds'} - \frac{dy}{ds} \frac{dx'}{ds'} \right\} ds\, ds'$$

round the axis of z; and similarly for the other axes.

18. Every elementary current in a given direction, as $i\,dx$, has at any point in space distant r from it the potential $\frac{i\,dx}{r}$, which being a vector or directed quantity, namely having the direction x, is with propriety termed the vector potential of the element. Maxwell indeed states distinctly (vol. ii. p. 267)

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that the vector potential stands in exactly the same relation to the elementary current in which the potential of a particle of matter stands to that particle—a statement which leads to all the consequences we now point out.

19. Again, the mutual potential energy of any two elementary currents $i\,ds$, $i'\,ds'$ being $\mu ii' \frac{\cos\epsilon}{r}\,ds\,ds'$, may be put in the form

$$\mu\left\{\frac{i\frac{dx}{ds}}{r}\,i'\frac{dx'}{ds'}+\frac{i\frac{dy}{ds}}{r}\,i'\frac{dy'}{ds'}+\frac{i\frac{dz}{ds}}{r}\,i'\frac{dz'}{ds'}\right\}ds\,ds'.$$

Now $i\frac{dx}{ds}\,ds$ is the component part of the current $i\,ds$ in the direction of x. If we assume, as we must, that the current, instead of flowing in an infinitely thin line, is (like a fluid of finite density) distributed uniformly over a small section perpendicular to ds, the x component will in like manner be distributed uniformly over the element of area $dy\,dz$. If we now write u, v, w for $i\frac{dx}{ds}$, $i\frac{dy}{ds}$, $i\frac{dz}{ds}$, when therefore u, v, w are the velocities of the current at ds parallel to the coordinate axes, and in like manner u', v', w' for $i'\frac{dx'}{ds}$ &c., we shall have for the mutual potential energy of the two elements the expression

$$\mu\left(\frac{u}{r}u'+\frac{v}{r}v'+\frac{w}{r}w'\right)dx\,dy\,dz\,dx'\,dy'\,dz'.$$

A similar expression holds for the mutual potential of every pair of elementary currents. We may therefore express the whole potential of any system of currents in the form of a sextuple integral

$$2T=\mu\iiint\iiint\left\{\frac{uu'+vv'+ww'}{r}\right\}dx\,dy\,dz\,dx'\,dy'\,dz';$$

or if

$$\iiint\frac{u}{r}\,dx'\,dy'\,dz'=F,$$

$$\iiint\frac{v}{r}\,dx'\,dy'\,dz'=G,$$

$$\iiint\frac{w}{r}\,dx'\,dy'\,dz'=H,$$

$$2T=\mu\iiint(Fu+Gv+Hw)\,dx\,dy\,dz.$$

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20. It would appear at first sight that we have omitted the energy of the action of each element *on itself*, which is *primâ facie* infinite. Exactly in the same way, in finding

$$\iiint \frac{mm'}{\sqrt{x^2+y^2+z^2}}\,dx\,dy\,dz$$

as the expression for the whole potential energy of a mass of matter, we appear to obtain the energy only of the mutual actions of each pair of particles, and not that of each particle on itself. But if the matter have at every point finite density, it is easily seen that the above expression gives us the whole potential.

In like manner, if the current u or $i\,dx$ be conceived as flowing, not through an infinitely thin line, but through a tube of finite section, so that the density, so to speak, of the current is finite at every point, the expression

$$\iint (\mathrm{F}u+\mathrm{G}v+\mathrm{H}w)\,ds$$

gives us the whole electrokinetic energy of all the currents in the field.

F, G, H will now be called the components of vector potential.

21. We have hitherto called the above expression

$$\mu \iiint (\mathrm{F}u+\mathrm{G}v+\mathrm{H}w)\,dx\,dy\,dz$$

the POTENTIAL energy of the system; and we have seen that it is deduced from our proposed law by methods applicable to potential of mass. We now assume, with Maxwell, that this energy, consisting, as he says it does, of "something in motion and not a mere arrangement," may be treated as kinetic energy for the purpose of applying to it the equation of Lagrange. By this means, as Maxwell has shown, the phenomena of induction are capable of explanation.

22. It is now to be observed that, as above mentioned, and as Maxwell points out, the vector potential of an elementary current in any direction, as x, stands in the same relation to the current as the potential of a mass of matter situated at the middle point of the element stands to that matter. It follows that the vector potential has all the properties of the potential of mass. Hence can be most easily deduced many most important theorems in electrodynamics.

For instance F, the vector potential of currents parallel to x, must satisfy the equation

$$4\pi\mu u+\nabla^2\mathrm{F}=0$$

(where u is the current in x), corresponding to Poisson's equation

$$4\pi\rho+\nabla^2\mathrm{F}=0,$$

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where ρ is density, ∇ potential of mass*. This equation occurs in the electromagnetic theory of light, giving one of the two expressions for u, from which arises the equation

$$K\mu \frac{d^2F}{dt^2} = \frac{d^2F}{dx^2}.$$

23. Again, it is a known property of the potential of mass, that if the potential of mass A has the same value as that of mass B at every point of a surface completely enclosing both, it has the same value at all points in space outside that surface.

In like manner we conclude that if F, the vector potential of the x component of a system of currents, has the same value as F′ (that of another system) at all points of a surface completely enclosing both systems, then F has the same value as F′ at all points beyond that surface. Hence can be easily deduced, by way of illustration, the theory of magnetic images as follows.

Let there be an infinite conducting plane, and let a determinate system of electric currents be suddenly generated parallel to and wholly on one side of the plane. Let u, v be the two components of current at any point in the system, F, G the components of vector potential of the system. By the principle of least kinetic energy a system of currents will be excited or "induced" in the plane, such as to make the whole energy the least possible consistently with the given currents in the given system. Let u', v' be the components of these currents at any point in the plane; F′, G′ the corresponding components of vector potential. Then for the whole energy we have

$$2T = \mu \iint \{(F+F')u + (G+G')v\}\, d\sigma \text{ over the system,}$$
$$+\mu \iint \{(F+F')u' + (G+G')v'\}\, d\sigma \text{ over the plane.}$$

And in order for T to be a minimum, given u and v at all points in the system, we must have $\frac{dT}{du'} = 0$, $\frac{dT}{dv'} = 0$ at every point in the plane. That is, $F = -F'$, $G = -G'$ at every point in the plane; and this being true at every point in the plane, must be true at all points in space beyond the plane.

The system of currents induced in the plane thus at all points beyond the plane exactly neutralizes the given system. It has, in fact, the same effect as the given system reversed in direction.

* The constant μ depends on the nature of the medium where the currents are situated, and stands in the same relation to the current as $\frac{1}{k}$ (where k is the specific inductive capacity of the dielectric) stands to free electricity.

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But the effect of the induced system at points on the same side of the plane with the given system is equal and opposite to its effect at the corresponding points on the opposite side of the plane. Consequently the induced system has at all points in space on the same side of the plane with the given system the same effect as the optical image of the given system formed on the plane as a mirror.

24. We have shown that the proposed law of force leads directly to the correct expression for the energy of any electrical system, whether that energy be termed kinetic or potential, and that by the shortest and easiest method. The further development of the theory would generally take the same form as in Maxwell's 'Electricity,' Maxwell's results being obtained generally by the application of Lagrange's equations to this expression for the energy.

LXI. *On the Transmission of Radiation of Low Refrangibility through Ebonite.* By Capt. Abney, *R.E., F.R.S., and* Col. Festing, *R.E.**

WHEN Mr. Graham Bell described his interesting experiments with the photophone, we were much surprised to learn that an effect was produced when sheets of ebonite of small thickness were interposed between the apparatus and the source of radiation; and it became a matter of more than curiosity to us to know what was the cause of the phenomenon, since photographic manufacturers were commencing to use ebonite in the construction of the dark slides for the camera.

We think we can demonstrate, however, that the ordinary explanation of transmission of radiant energy can account for the phenomenon. Dr. Guthrie kindly furnished us with a sheet of ebonite, through which the action of a beam of radiation on a selenium cell was most marked; and we accordingly first experimented with that. A photographic spectroscopic apparatus was employed, of the form we have already described in other papers; and the compound of silver was used which is sensitive to all parts of the spectrum. At first we employed only one prism, and used the sun as a source of illumination; and here it may be parenthetically remarked that on the evening when we made our first experiments the wind was blowing from the north-east, and there was a clear sky. Half the slit was covered up, a piece of ebonite placed in front of the other half, and a plate exposed to the action of the spectrum of the radiations (if any) coming through this thin layer of apparently

* Communicated by the Physical Society, having been read at the Meeting on April 9.

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opaque matter. An exposure of three minutes was given; the exposed half of the slit was then closed and the other half opened, and a spectrum taken through a solution of bichromate of potash $\frac{1}{10}$ inch thickness. This bichromate was used to prevent the too energetic action of the more refrangible rays, which illuminated the prism and would have caused a veil over the plate. Half a minute's exposure was given. The plate, on development, revealed that rays of very low refrangibility had passed through the ebonite, commencing at W.L. 12,000 and extending as far as W.L. 7500; the point of maximum intensity was situated at about 9000. The photographs were on a small scale, but sufficed to show the absorption of the ebonite. On the next day we had intended to repeat the experiments with two or three prisms; but the wind had shifted, and the solar spectrum was absorbed as far as about 9000, showing the presence of aqueous vapour. It was therefore useless to experiment further with the sun as a source of radiation; so we used the water of the positive pole of an electric light as a source. It will be seen that the spectrum through ebonite extends to about W.L. 15,000, and then terminates.

The next point to determine was as to the quality of the beam coming through the ebonite. This we determined as follows—first by placing a piece of ebonite in contact with the photographic plate and throwing an image of the points on it, and thus getting an impression, and then, by a simple arrangement, removing the ebonite to a distance of 1 foot, and allowing the beam to traverse it, and securing another image on a different plate. The photographs showed that the rays are very much scattered in their passage through the ebonite, no distinct image being formed in the latter case, though it was sharp and defined in the former. The amount of scattering it seemed desirable to know. For this purpose the collimator of the spectroscope was used and no prism, the image of the slit $\frac{1}{20}$ inch wide was focused on the focusing-screen of a camera, and a piece of ebonite was placed in contact with the plate, and exposure made. This piece was removed and another piece inserted $2\frac{1}{2}$ inches in front of the plate, and another exposure given. The diffusion was most marked: a line $\frac{1}{20}$ inch broad was diffused over a space $\frac{1}{4}$ of an inch, most intense, of course, in the centre. By subsequent experiment it was shown that an exposure of three times the length of that given in the first case was necessary to cause the central portion of the band in the second case to correspond in intensity with that of the image of the slit in the first case. With two pieces of ebonite in contact with the plate six times the exposure was required to give the same intensity as with only one plate of ebonite intervening. Hence we may say that the coefficient

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of absorption of a plate of ebonite $\frac{1}{67}$ of an inch in thickness $=1{\cdot}8$; and a calculation will show that any rays which can penetrate through $\frac{1}{8}$ of an inch of ebonite will only have an intensity of $\frac{1}{1650000}$ that of the resultant beam, without deducting any thing for the scattering of light. In fact, with the electric light and a wide slit an hour's exposure produced no effect on the photographic plate when ebonite $\frac{1}{8}$ in. in thickness was placed before the slit. It must, however, be remembered that ebonite varies in quality; sometimes the outside alone is black, the inner portions resembling gutta percha in colour. With specimens of this sort a greater thickness could no doubt be traversed than $\frac{1}{8}$ inch. In such a case, however, we doubt if the substance would be true ebonite.

In a communication to 'Nature,' Messrs. Ayrton and Perry show how they determine the refractive index of ebonite by an arrangement with the telephone. They use a prism; and we should judge by the figure they give that the thickness of ebonite traversed must be about $\frac{1}{4}$ of an inch; so that the radiations transmitted must be very small. We may remark that the direction of a beam of light issuing from a prism formed of a turbid medium would not have its maximum intensity in the true direction of refraction; it would be slightly displaced. Mr. Preece, in a recent communication to the Royal Society, remarked that some ebonite he tried was as transparent as rock-salt; and so it is if a thin-enough layer be taken; and we think that it was the minute layer that was taken that caused this expression to be used. He also stated that another sample equally thin was perfectly opaque to radiation. Through his kindness we were able to experiment with the identical samples to which he refers. The "transparent" specimen behaved as that we have already described; the opaque one showed that the radiations were more scattered in their passage through it. We may state that, by examining the thin ebonite with which we first experimented, we could see a trace of the sun's image through the material, and very faintly through two layers. The radiations of low refrangibility were evidently more copiously passed, since when an image of the sun formed by a lens was caused to fall on a piece of paper and a sheet of thin ebonite interposed, if the eye or hand was placed at the focus considerable warmth was felt.

It became interesting to know whether the ebonite was merely a mechanical mixture of sulphur and india rubber or a chemical combination. Placing a piece of stout india rubber, about the same thickness as the ebonite, before the slit of the spectroscope, and with an exposure of ten minutes, no vestige of an image was found on development of the plate. This

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was evidently owing to the great scattering of the rays by the substance. The india rubber being laid in contact with the plate, and an exposure made through it, showed that it was transparent to all rays from 10,000 to 5000. The absorption-spectrum therefore differed; and it is evident that in ebonite the india rubber is chemically changed in composition.

The conclusion to be drawn is, that ebonite, when of small thickness, transmits to some extent the rays of low refrangibility.

LXII. *Notices respecting New Books.*

Eine neue Form elektrischer Abstossung. (*A new Kind of Electric Repulsion.*) *By* Dr. Eugen Goldstein. Berlin: Julius Springer. 1880.

THE work before us is an octavo of nearly 200 pages, with six plates of lithographed figures, forming the first part of Dr. Goldstein's 'Researches on the Electric Discharge in Gases.' It contains minute and careful descriptions of experiments which have occupied the author for eight years, and discussions of the conclusions to which the experiments lead. The final conclusion arrived at is, that the phenomena observed are not capable of explanation in the present state of our knowledge—that they are not to be explained by the known laws of electrostatic or of electrodynamic repulsion, nor by the mutual mechanical action of molecules. So far, therefore, the ground has been cleared by showing to what the phenomena are *not* due; and further investigations on this subject from the skilful hands of Dr. Goldstein will be looked for with much interest.

In the Introduction we are reminded of the appearance of the electric discharge in an ordinary vacuum-tube (of simple cylindrical form) filled with air or some other permanent gas. At atmospheric pressure the discharge consists of a thin thread-like spark (the "trait de feu") and the surrounding glow or "aureole." As the tube is gradually exhausted the "trait de feu" disappears first, and the aureole remains and expands and fills the tube with light, in which are usually distinguished two portions—the positive and negative "glows." From the positive pole or anode there proceeds a stream of light expanding towards the opposite pole, which in air is of a reddish colour, termed the positive discharge; it shows, as soon as the exhaustion has reached a certain point, regularly alternating maxima and minima of light—the so-called stratifications. The number of the stratifications varies not only with the density and nature of the gas, but also with the width of the tube and the distance between the electrodes. The positive light however, is, not a necessary portion of the discharge; and its presence or absence depends on the ratio of the distance between the electrodes and the smallest diameter of the tube at right angles to the line of discharge. If this ratio is not greater than 2, then (at small densities) there is no positive discharge.

From the negative pole or kathode there proceeds also a luminous discharge, which also consists of layers (or stratifications), whose

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number, however, is always the same, not varying either with the nature of the gas or its density, nor with either the dimensions of the tube or the position of the electrodes. There are always four layers of the kathode light: the first is of a chamois-yellow colour; the three outer ones are blue. Like the layers of the positive light, those of the negative light expand as the pressure decreases. When, at pressures of less than 1 millim., the negative light extends through the whole tube and plays upon the opposite wall, it excites there phosphorescence, of colour depending on the nature of the glass. Thus the German glass commonly used for vacuum-tubes phosphoresces with green light, the intensity of which increases as the pressure decreases; while at the same time the optical intensity of the kathode-light itself decreases until at last the eye can scarcely perceive that any light at all is emitted by the kathode. The negative light and the positive light do not, at small pressures, occupy distinct portions of the tube, but may each fill the whole tube. The presence of the brighter positive light thus increases the difficulty of observing the phenomena taking place at the negative pole; so that for successful experiment it is necessary to get rid of the positive light altogether. This can be managed by having the distance between the electrodes not greater than twice the diameter of the tube. The smaller the distance between the electrodes, the higher the density at which the positive light disappears. It is sometimes desirable to place the anode close to the kathode. The radiation of the negative light from the kathode takes place, as Hittorf showed, without reference to the position of the anode—not even ceasing when it reaches the anode, but extending beyond it through the whole of the tube. Thus, if both poles are placed at the same end of a cylindrical tube, the kathode-light stretches right through the tube till it reaches the end which contains no electrodes, where it excites phosphorescence in the glass.

Plücker and Hittorf have already shown that the kathode-light radiates in straight lines. If the kathode be a straight wire, and if the tube contain a second similar wire parallel to the first, a sharp shadow of the second is seen on the phosphorescent surface excited by the discharge from the first.

This Introduction occupies 16 pages; and the work itself is divided into four parts.

In the First part the nature of the phenomena observed by Dr. Goldstein is expounded. In the simplest form of the experiment, a cylindrical tube contains at one end two parallel wires, one or both of which may be connected with the negative pole, and at the other end of the tube a wire to serve as anode. If one only of the negative wires is excited, the kathode-light from it excites green phosphorescence in the glass all around, on which is seen the sharply-defined shadow of the unexcited wire. If, now, the second negative wire be excited at the same time as the first, there appear immediately two dark spaces in the green phosphorescence of the glass, each so situated as to resemble nearly an enlarged shadow of one of the wires produced by the discharge from the other. The dark surfaces may be described as oval, but with straight sides

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parallel to the wires. The form, magnitude, and position of the dark surfaces are independent of the position and magnitude of the anode, and of the direction in which the *positive* discharge takes place, but may be largely altered by varying the form, magnitude, and relative position of the two kathodes. These changes are minutely followed through a large number of widely varied experiments. The dark surfaces are in all cases such as can be explained by a repulsion exerted by each electrode on the kathode-rays proceeding from the other, so that the (comparatively) dark spaces are due to the non-production of phosphorescence on those portions of the glass wall not reached by the deflected rays. It should be noted that the boundary of the dark surfaces possesses increased brilliancy, so that the dark surfaces appear bounded by a sharply-defined line of light.

If the kathodes are of unequal thickness the dark surfaces are of unequal breadth, that nearest the thicker electrode being the largest.

The Second part of the work is devoted to the examination of the cause of these phenomena. First of all, it is shown, by making one of the kathodes of a twisted aluminium wire, that it is only light from the further kathode which is deflected from each dark surface, and that each is illuminated by light from the nearer electrode, just as when it alone was excited. It may be explained, in passing, that the phosphorescence excited by the discharge from a twisted wire is not uniform, but exhibits a spiral line of greater brilliancy.

If a diaphragm having a small square hole in it be placed between two parallel kathodes with its plane at right angles to their plane, then, when only one is excited, a small pencil of rays passes through the opening and forms a phosphorescent speck on the glass, which is crossed by a fine dark line, the shadow of the second wire. If the second wire be also excited, the phosphorescent speck divides into two, which separate from each other to a distance of some 12 millim. on the walls of the tube, thus clearly proving the repulsion exerted upon the rays by the second kathode as they pass it. In order to determine whether the pencil is deflected as a whole, or whether it is bent into a curve, or whether it is so bent as to form a sharp angle at the point nearest the electrode, the following experiment was made:—A movable electrode, so arranged as to emit a small concentrated cone of light, is hung on to the bent end of a wire forming one of the kathodes, and in consequence of its weight always hangs vertically, and emits a cone of light whose axis is vertical. The other kathode is a wire crossing the tube. By slightly altering the position of the tube the cone of light may be made to impinge on the second kathode, or to pass close to it on either side, or at a little distance from it. It is found that when both kathodes are excited, the cone of rays is bent sharply at an angle of about 30° in passing the horizontal kathode, or is divided into two portions forming an angle of about 70° with each other, if it is allowed to impinge on the horizontal kathode. The deflection is found to be greater the closer the ray passes to the deflecting kathode.

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In the Third part of the work, Dr. Goldstein investigates the effect upon the deflection thus shown to take place, of varying the nature of the gas in the tube, or of the metal of which the electrodes consist, the density of the gas, the intensity of the charge communicated to the kathodes, the source of the electricity, and so on. It is found that neither the nature of the gas, nor its density, nor the nature of the metal employed for the electrodes, has any influence upon the phenomena observed; and also that the intensity of the charge is of no consequence provided that the two kathodes are equally charged, as they must be if placed in good metallic communication with each other. If, however, they are connected by a bad conductor, such as a moistened thread, the dark surfaces produced are unequal in size, the largest being that nearest the kathode which is most strongly charged. The repulsion exerted by a kathode is thus increased by increasing its electric charge; and, on the other hand, the *stiffness*, so to speak, of the rays emitted by a kathode is increased by increasing its electric charge.

In the Fourth part of the work, the different theories of the electric discharge are examined, in order to see how far they can explain the new phenomena. Three kinds of repulsion are known—that due to mechanical action (such as takes place in the impact of ponderable masses), electrostatic repulsion, and electrodynamic repulsion.

In adopting the mechanical hypothesis, we must suppose that molecules of gas charged with electricity fly off from each kathode, that they come into collision, and that the two motions combine to a resultant motion whose direction will be that of the deflected ray. To decide this question, a cylindrical tube was taken provided with a kathode at one end, and a second kathode and diaphragm some distance from the first in the side of the tube, from which, when excited alone, a sharply-defined pencil of rays falls on the opposite side of the tube. It is found that the rays from the terminal kathode are deflected on passing the lateral kathode, but that the rays from this second are not affected by the supposed impact of the molecules forming the first bundle of rays.

It is further clearly shown that the phenomena are not consistent with the known laws of the mutual action of currents; so that the electrodynamic hypothesis must be rejected.

The third hypothesis is that the action is an electrostatic one, between the electricity with which the one kathode is charged and the like electricity residing in the molecules of the stream from the other kathode.

According to what may be termed the "convective theory," the electric discharge is a sort of convective process, consisting in the motion of molecules charged with electricity, which bring about the transference of electricity from the kathode in the direction of the stream of rays. This convective theory is the one which has attracted most attention. The particular form of the theory expounded by Crookes assumes that the gas-molecules charged with electricity are driven off from the kathode as a sort of wind which drives the uncharged molecules before it; so that, surrounding the

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kathode, we have a broad space filled by molecules which are not in collision with each other. Only when the molecules come into collision with each other beyond this space do they become luminous, so that the space surrounding the kathode is non-luminous. Observation, however, shows that this space is *not* non-luminous, but emits light of a deep blue colour, to produce which the extremely small quantity of gas present must possess a high emissive power. Moreover, immediately surrounding the kathode there exists, as already noted, the *first* layer of a bright yellow colour. Further, Dr. Goldstein observes that molecules thus driven off from the kathode could only in special cases pursue a rectilinear path, since it is clear that a molecule charged with electricity, which enters into motion in consequence of its presence in an electric field, must describe a line of force. If the surface of the kathode were spherical, such lines of force would, it is true, be straight; but in the usual case, when the kathode consists of a straight thin wire, the lines of force would be hyperbolas whose foci would be the ends of the wire. The shadow of a straight wire placed near to and parallel to a straight kathode of slightly greater length would therefore considerably exceed the wire itself in length. Observation shows, however, that the shadow is almost exactly of the same length as the wire itself, and that the rays are actually rectilinear and at right angles to the kathode.

As already stated, Dr. Goldstein rejects all three theories, as insufficient to afford full explanation of the facts.

Williamson's *Integral Calculus.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In the flattering reference to my investigations in kinematics contained in your review of Williamson's 'Integral Calculus' in the April number, there is one misapprehension that I feel bound to remove. The results obtained by Messrs. Leudesdorf and Kempe are spoken of as suggested by my earliest one. They were, however, quite independently arrived at; indeed in publication, though not, I think, in composition, Mr. Leudesdorf's theorem had the priority of mine by two months.

I am, Gentlemen,

Yours faithfully,

Queen's College, Oxford, May 11. E. B. ELLIOTT.

LXIII. *Intelligence and Miscellaneous Articles.*

NOTE BY MR. R. SHIDA ON HIS ARTICLE (PHIL. MAG. DEC. 1880), "ON THE NUMBER OF ELECTROSTATIC UNITS IN THE ELECTROMAGNETIC UNIT*".

ON page 360, Phil. Mag. for May 1881, Dr. C. R. A. Wright has made the following statement:—"That the E.M.F. of a Daniell cell is somewhat less when generating a current than when no current passes has already been shown to be the case by Waltenhofen

* Communicated by Sir William Thomson.

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(*Sitz. Akad. Wien*, xlix. 2, p. 229) and by Beetz, using a modification of the method of compensation (Pogg. *Ann.* cxlii. p. 581, 1871). Apparently, however, physicists generally are under the impression that the diminution in E.M.F., or "polarization" of the cell, thus produced is practically inappreciable: thus, for instance, within the last few months a determination of the value of v has been made in Sir W. Thomson's laboratory by R. Shida, by measuring the E.M.F. of a gravity Daniell, first, in electrostatic units (no current being generated), and then in electromagnetic units (a moderately powerful current being set up); the which method evidently can only be applied if the difference in the E.M.F. according as a current is generated or not is negligible (Brit. Assoc. Reports, 1880, p. 497). The same kind of remark applies to several other investigations published during the last few years."

With reference to the above remark on my paper, I have to state that I knew quite well that the *calculation* of an E.M.F. may be vitiated by "polarization" (a well-known phenomenon), but that when the external resistance is great compared with the internal resistance of the cell, as was the case in the determination of the E.M.F. of a Thomson gravity Daniell, described in my paper, the alteration in the E.M.F. due to "polarization" is so small, if any thing, as to be negligible. It would have been better, however, had I not omitted to mention that what I really depended upon was the measurement (performed both electromagnetically and electrostatically) of the E.M.F. of the cell *while the current was actually flowing through the tangent-galvanometer*. The result confirmed perfectly the calculation according to "Ohm's law" on the supposition of constant E.M.F. and constant resistance, which I gave in my paper, and which I adopted only after this confirmation.

ON THE PRINCIPLE OF THE CONSERVATION OF ELECTRICITY.

BY G. LIPPMANN*.

The quantity of matter and the quantity of energy are not the only magnitudes which remain constant: the quantity of electricity enjoys the same property. If we consider any (electrical) phenomenon in its entirety, we perceive that the distribution of the electricity may change, but that the sum of the quantities of free electricity never varies. If the electric charge experiences a positive variation at certain points, it experiences at other points a negative variation; and *the algebraic sum of all the simultaneous variations of the charge is always zero*. The sum of the quantities of free electricity is then constant, since its total variation is always equal to zero. This law, which I call the *principle of the conservation of electricity*, extends over all the phenomena hitherto studied; it results from old and well-known experiments of which the mention is sufficient. Thus, in the case of the sharing of a charge between two bodies, we know that the total charge remains the same after redistribution as before. It is the same in the case of friction: we

* Translated from the *Comptes Rendus*, t. xcii. No. 18, p. 1049, May 2, 1881.

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know that the charges acquired by bodies when rubbed have an algebraic sum of zero. It is the same, again, in the case of electrification by induction and in that of the action of voltaic batteries. I shall admit as a principle this fact, which has been verified for all known electrical actions*. In the memoir which I have the honour to submit to the Academy, I have set myself to translate into analytical symbols this fact, in order to draw new conclusions therefrom.

Let us call x and y two independent variables upon which the quantity of electricity which a body receives depends: x may be, for example, the potential which a body acquires, and y its capacity or any thing of which the capacity is a function, a length, a pressure, a temperature, &c.

Let dm be the quantity of electricity received by the body while x increases by dx and y by dy. We may, without assuming any other conditions, write

$$dm = \mathrm{P}dx + \mathrm{Q}dy,$$

P and Q being two functions of x and y.

I assert, then, that the principle of the conservation of electricity is expressed by the condition that dm be an exact differential. For let us in thought divide any system in which an electric phenomenon is occurring into two parts, A and B. Let a and b be the variations of charge experienced simultaneously by these two portions: by virtue of our principle we must have $a+b=0$. In the case in which A passes through a complete cycle of changes, so that its final state is identical with its initial state, we shall have $a=0$, and therefore $b=0$. This last equation may be written $\int dm = 0$. Now, that an integral such as $\int dm$ may be zero for a complete cycle, we know that it is necessary and sufficient that dm be an exact differential, which is again implied by the condition of integrability that

$$\frac{\partial \mathrm{P}}{\partial y} = \frac{\partial \mathrm{Q}}{\partial x}. \qquad (\alpha)$$

Such is, then, the general analytical expression of the Principle of the Conservation of Electricity.

The Principle of the Conservation of Energy is likewise expressed by a condition of integrability. We thus obtain two *distinct* equations, whose simultaneous application to different known phenomena makes us foresee the existence and importance of new phenomena. I shall have the honour of submitting to the Academy some examples of this application.

* The enunciation may be repeated in the following form:—*Whatever may be the phenomena which are produced between the parts of a system, the total electrical attraction exercised upon this system by an infinitely distant electric particle remains constant.* If we were to employ the attraction exercised by an infinitely distant electric particle to measure quantities of electricity, this measurement would be made by electric weighings analogous to the weighings of the chemist; and the conservation of quantities of electricity could then be verified in the same manner as the conservation of quantities of matter.

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ON A NEW FORM OF GALVANOMETER FOR POWERFUL CURRENTS. BY PROFESSOR C. F. BRACKETT, COLLEGE OF NEW JERSEY.

The very powerful currents produced by large dynamo-machines are not easily estimated by the appliances usually found in the physical laboratory.

The various forms of the electro-dynamometer, the cosine-galvanometer, and some other special forms of apparatus may of course be employed with satisfactory results. None of these instruments being in the collection at Princeton, it was determined to construct a galvanometer which should obey the law of tangents and yet not be so large as to be unwieldy. In order to do this, recourse was had to the differential principle. The construction is as follows:—

Two stout hoops of copper or brass of different diameters are very exactly turned in the lathe. They are then each cut open at one point and joined to each other concentrically, by soldering with hard solder, between the ends on one side of the cut, a piece of metal having the same cross section as that of the hoops, and of suitable length. At several other points are inserted between the hoops pieces of hard rubber of proper thickness, which serve to keep them truly concentric.

The free ends of the system thus arranged, and set upright on a proper base-board, are joined to binding-screws.

It will be seen that the differential action on a needle placed at the centre, or on the axis of the hoops pasing through their centre, depends on the different distances of two equal and opposite currents. It is evident also that the instrument may be used as a simple tangent-galvanometer.

Thus, if we call the free ends of the hoops A and B respectively, and the point of juncture C, by joining up a circuit through A and B we get the differential action; but by joining up through C and A or B, the action is that of a simple tangent-galvanometer. If r and r' represent the radii of the outer and inner hoops respectively, the ordinary formula becomes

$$I = \frac{rr'}{2\pi(r-r')} \times H \tan d$$

when the instrument is used differentially.

An instrument has been constructed at the J. C. Green School of Science for its physical laboratory, of dimensions as follows:—

Diameter of outer hoop	10·9 centim.
Diameter of inner hoop	9·96 „
Width of each hoop	2·3 „
Thickness of each hoop.....	0·35 „

The theoretical constant of this instrument agrees very closely with that ascertained experimentally by means of the voltameter. The needle, which is usually suspended in the centre of the hoops, may, if desired, in order to measure exceedingly powerful currents, be moved along their axis to any required distance on one side. The constant is in that case easily found by an obvious modification of the formula.—Silliman's *American Journal*, May 1881.

Princeton, March 10, 1881.

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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XI. FIFTH SERIES.

LXIV. *The Existence of the Luminiferous Æther.* By ERNEST H. COOK, *B.Sc., Assoc. R.C.Sc., Lecturer upon Experimental Physics at the Bristol Trade and Mining School**.

IN the number of this Journal for April 1879 there appeared a paper with the above title, in which the author endeavoured to show that the assumption of an all-pervading æther in order to account for the phenomena of optics was unnecessary. In place of the æther as the vehicle of wave-motion, it was suggested that the particles of matter themselves possessed the requisite properties, and were therefore able to take up and transmit the motion producing light. Since that paper was written, however, many advances have been made in the domain of molecular physics. Some of these have a direct, and many more an indirect, bearing on the subject. At the present time, therefore, it may be of interest to examine the more important of these researches, and try to glean from them the conclusions to which they lead. In doing this the endeavour will be made to state the case in as fair and impartial a manner as possible. Conclusions will be drawn from a series of facts, and not from isolated ones, and, whenever possible, will be confirmed by independent experimental evidence. In this way, although we may not be able to arrive at the whole truth (owing to a deficiency of experimental evidence), yet we stand the best chance of extracting the greatest amount from the knowledge we do possess. But the examination of the varied phenomena presented to us by the labours of experimental philosophers is a task of such magnitude, that we cannot hope to do more than notice the more important of the researches. Let us then begin with

* Communicated by the Author.

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The Radiometer.

Five or six years ago, when Mr. Crookes announced his important discovery, it was thought that the death-blow had been given to the undulatory theory of light, and the emission theory of Newton established in its stead. Bennett (Phil. Trans. 1792) had previously tried, by concentrating the light upon a balance supported by a single spider's web, to produce motion, but without success; no motion could be produced by means of the light-ray. But in the radiometer apparently continuous rotation of the fly was produced by the action of the light-rays. The inventor himself, whilst carefully guarding himself against pinning his faith to any theory, thus speaks of his discovery at a time when the correct theory of the action had not been propounded:—"But light, in some respects the highest of the powers of Nature, has not been hitherto found capable of direct conversion into motion; and such an exception cannot but be regarded as a singular anomaly. This anomaly, the researches which I am about to bring before you, have now removed; and, like the other forms of force, light is found to be capable of direct conversion into motion, and of being (like heat, electricity, magnetism, sound, gravitation, and chemical action) most delicately and accurately measured by the amount of motion thus produced." (Lecture at the Royal Institution, Friday evening, Feb. 11th, 1876.) Shortly after the lecture in which these sentences appeared was delivered, Mr. G. J. Stoney published his explation of the action of these instruments in two papers which appeared in the Phil. Mag. for March and April 1876. After subjecting this theory to the most careful examination, it was finally accepted by Mr. Crookes as giving a complete and satisfactory explanation of the matter. Viewed in the light thus thrown upon it, it will be seen that the rotation is not due to the action of the light-waves as such, but that the unequal heating of the disk and glass envelope gives rise to the production of streams of molecules, which, reacting on the vanes, produce rotation. The full investigation of this theory will be found in the two papers by Mr. Stoney before referred to, and also in the following:—Two additional papers by Mr. Stoney—one "On Penetration" (Phil. Mag. December 1877), and one "On Polarization Stress in Gases" (Phil. Mag. December 1878); and a paper by Mr. G. F. Fitzgerald, "On the Mechanical Theory of Crookes's Force" (Phil. Mag. January 1879). An account of the rigorous experimental tests to which Mr. Crookes subjected it will be be found in the 'Proc. Royal Society,' November 16, 1876, and April 26,

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1877, and also in the pages of the 'Chemical News' for 1879.

Stripped of its mathematical accompaniments, this theory may be briefly stated thus:—If two surfaces at different temperatures be opposite to each other with a gas between them, there exists a force or stress tending to separate them. This is almost self-evident in the case of a volatile liquid resting on a hot surface; for the stress then has to support the weight of the drop in addition to the atmospheric pressure. If one of these surfaces be movable, we should be able to produce motion, and, under certain conditions, rotation, by the action of this stress. This is just what Mr. Crookes has done. In the radiometer the blackened surface of the vane becomes slightly heated, owing to its absorption of the solar rays. Excess of pressure between it and the sides of the containing vessel is thus produced, resulting in the rotation of the fly. The blackened surface of the vane becomes the heated surface, the sides of the containing vessel the cooled. Now, if the motion is produced by the mutual action of the two surfaces, and in the radiometer the heater is movable while the cooler is fixed, if we make the cooler movable and fix the heater, then we ought equally to get rotation. This is the difference between the otheoscope and the radiometer: in the first the heater is fixed and the cooler movable; in the second the cooler is fixed and the heater movable. Mr. Crookes has brought forward abundant experimental proof of the existence of this stress; and we have now to consider how it is brought about. Imagine two surfaces of unit area marked out on the heater and cooler, then the gas between them will be in active molecular motion, but stationary as far as regards convection-currents and wind. Suppose perpendiculars drawn to the two surfaces from the boundaries of these surfaces. Then we shall form a tube whose two ends are at temperatures T_1 and T_2, and whose sides (let us imagine) are perfect reflectors of molecules. Since the gas is stationary, we must have that across any section of such a tube equal numbers of molecules are passing in opposite directions at the same time. But the average velocity of those passing towards the cooler is greater than the average velocity of those passing towards the heater, because of the difference in temperature between the two surfaces, and, moreover, bears a known ratio to this difference. Thus in such a gas a state of stress would be set up which would produce a mutual repulsion between the two surfaces. These are the aspects of the subject which are really important, the further investigation of the motion of the molecules, *i. e.* their impact, does not concern us at present. It is sufficient

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for us that a *molecular motion of a particular kind produces the rotation of the fly*. That this is the true explanation of the phenomena has been abundantly proved by the experiments of Mr. Crookes and others. Now, as the luminiferous æther is all-pervading, and is therefore present in the exhausted cases of the radiometers, we ask where does its influence come in in the phenomenon? Does not the acceptance of this theory virtually ignore the existence of the æther? Only one way out of the dilemma is left, viz. to say that the æther is there, but that it does not affect the motion of the molecules in the least. Here, as in every other case where its influence is likely to be felt, we find it exerting no influence, and behaving like an inert substance. But are we, when, as in this case, we call upon the believers in the existence of this substance to tell us how it will affect a certain action, to be content with the bare statement that it exerts no action? We are entitled to ask Why? And what are the properties of the substance in virtue of which it is inert? But if the æther be present, it must make its presence felt in some way or other. In fact, Prof. Challis has attempted to explain the phenomena of the radiometer simply by the motion of an æthereal current. He says:—"I assume that the radiant light or heat which is incident on the vanes, being thereby converted into heat of temperature, causes the atoms in a thin superficial stratum to be displaced from their neutral positions, and in a greater degree on the blackened side than on the other, on account of the greater accession of temperature on that side. . . . . This is a *real* change of condition of the vanes. . . . . Every such superficial disturbance, however caused, gives rise to *a steady circulating* ætherial current, in which the pressure varies so as to be always less the greater the velocity; and consequently atoms immersed in such a current will be dynamically acted upon by reason of the variation of pressure. In this instance the course of the current is from the blackened to the opposite surface, because, on account of the greater expansion of the vane on the warmer side, the channel for the currents diminishes and the velocity increases towards the bright side, and accordingly the dynamical action is the same as if the vane were *pushed* on the blackened side"*. Here, then, we have very clearly expressed the action which the æther would exert if present. Taking now the molecular-pressure theory, let us see how this action affects the movements of the molecules. In the æthereal current "the

* "A Theory of the Cup-shaped Radiometer with both Sides Bright." By Prof. Challis, M.A., F.R.S. Phil. Mag. April 1877.

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pressure varies so as to be always less the greater the velocity," and the velocity is greatest towards the bright side; and hence the pressure is greatest towards the blackened side. Moreover, "atoms immersed in such a current will be dynamically acted on;" and in consequence of this action the vane is pushed on the blackened side. But if the molecules of the blackened side of the vane are pushed, *so also are the residual molecules of air which are leaving that side in their passage to the cooler.* Now the essential point in the production of Crookes's force is, that the molecules which leave the heater do so with a greater velocity than those which leave the cooler, and thus possess a greater *vis viva.* The presence of the æther therefore, by exerting this dynamical action, *retards and resists the motion of these molecules.* We are thus bound to assume that the cause producing Crookes's force is a sufficiently powerful one to overcome the resistance which is offered by the dynamical action of the æther.

It therefore appears that, if we assume the existence of this æther, the action of the radiometer is caused by the operation of two directly opposite forces—one, the greater, being produced by the movement of the molecules of the residual gas, and the other by the pressure produced by the circulation of the æther. The same cause, viz. the heating of the vane, produces both forces; but the effect of this heating upon the molecules of the residual gas, we must suppose, is much greater than upon the æther. It appears, therefore, that this molecular-pressure theory of the action of the radiometer must be modified in the manner indicated, in order that the disturbing effect of the vehicle of light may be taken into consideration Is it not more probable that there is no æther present?

2. *Optical Properties of Chemical Substances.*

That the crystalline arrangement of bodies produces a great and decided effect upon a ray of light passing through them is a fact perfectly well known. In fact, to it we owe the phenomena of double refraction. The general bearing of this effect upon the question under consideration has already been considered in the paper referred to above (Phil. Mag. April 1879). But there are certain special portions which will require a little further consideration. Some of these are not recent investigations, but are nevertheless very important.

Phenomena presented by Biaxal Crystals.—It is a well-known fact that the relative positions of the optic axes varies with the temperature; in fact, in some crystals, when heated, not only the inclination of the two optic axes to each other, but also the plane in which they lie is liable to change. Now

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here we undoubtedly have a well-marked effect produced by heat on the substance, whatever it is, which transmits the undulatory movement. Assuming the existence of the luminiferous æther, we explain this as follows:—The heat produces a more rapid oscillating movement among the particles of the body, the amplitude of the vibrations which the particles execute being at the same time increased. The freedom of the æther particles in which the others are immersed to oscillate is changed owing to this; and this change is of such a nature as to alter the inclination of the optic axes to each other. The only part of this explanation which it is difficult to comprehend is the most important part of it, viz. why the *oscillation of the molecules of the body should alter the elasticity of the containing medium*. When we remember the relatively large space which exists between the molecules of bodies, and therefore the large volume of the contained æther compared with the size of the vibrating molecule, it is difficult to understand the reason why so small a vibrating mass should affect so large a containing one. Again, we must remember that the effect of the heat is simply to increase a previously existing motion, and not to call into action a new species of motion. It is therefore less likely that this increased motion should affect the containing medium. Another consideration which makes it difficult to accept this explanation is, that one of the best-marked properties of the æther is its extreme mobility, which would therefore allow of the vibration of molecules in it without resistance and also without affecting it. The acceptance, then, of this explanation is attended with various difficulties.

Assuming, however, that the transmission of the undulatory movement of light takes place by the vibration of the particles of the body, the effect of the alteration of temperature is very natural, and, in fact, is just what would be expected. The increased oscillation of the particles by a rise in the temperature causes an alteration of a certain kind to occur in the mode of vibration possible to those particles. It is unnecessary for us to inquire what the precise nature of this alteration is, as it is easily seen that *an* alteration is possible. The change may be analogous to the change in the clang-tint of a note produced by the admixture of different harmonics with the same fundamental.

Secondly, the researches of Brewster have shown that some crystals possess two axes for light of one colour, but only one for light of another. Thus Glauberite possesses two axes inclined to each other at an angle of 5° for red light, but only one for violet. These experiments may therefore be taken to prove that for red light the molecules of the crystal are symmetrically arranged

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around two lines in the crystal, while for violet light the arrangement is only around one such line. (It does not make any difference, so far as this is concerned, whether we consider the æther or the molecules concerned in the transmission of the light.) Now we are thus brought face to face with one of two things, viz. either the different vibrations of the two kinds of light produce differences in the molecular arrangement of the crystal, or the molecular arrangement itself is different for red to what it is for violet light. That a change takes place in the arrangement of the molecules when the different kinds of light fall on the crystal is most unlikely; and thus we are left with the second alternative. The intermolecular æther, then, must be so constituted in these crystals as to be capable of vibration for long waves (red light) in one way, and for short waves (violet light) in another. How is this constraint brought about? We are told by the *presence* of the molecules of mundane substance whose molecules are scattered about in this æther just as the stars are in space. It is contrary to our notions of nature to imagine that the mere presence of the molecules is going to affect the elasticity of the substance in this complicated manner. If, however, we imagine the molecules of the crystal concerned in the propagation of light through it, we must make a slight additional assumption, viz. that different groups of molecules are concerned in the transmission of different kinds of light. Then we have that the groups concerned in the transmission of the red waves are differently arranged from those which transmit the violet. Thus in the case of the crystal cited we have that the groups of molecules transmitting the red are symmetrically arranged around two axes, while those transmitting the violet are thus arranged around one axis only.

Thirdly. We take the experiments of Herschel by which he proved that the axes are inclined to each other at different angles for light of different refrangibilities. Thus in Rochelle salt the axes for violet light are inclined to each other at 56°, and for red light at 76°. In explanation of this, Herschel says:—"The directions within the crystal of the two axes of double refraction, or the 'optic axes,' stand in no abstract *geometrical* relation to those of the angles and edges of its 'primitive form,' or to its axes of symmetry. They are resultant lines determined by the law of elasticity of the luminiferous æther within its substance as related to its crystalline form, and *to the wave-length of the particular coloured ray transmitted*"*. In reference to this, it is only necessary to remark that here we have to make an additional assumption with regard to the

* Herschel, 'Familiar Lectures,' p. 398.

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action of the molecules of material bodies upon the æther, an addition which makes a greater strain upon the imagination. It is necessary not only to assume that the *proximity* of the æther-particles to those of the crystal causes a difference in the elasticity of the æther, but also that there is no "abstract geometrical relation" between the shape of the crystal and the axes of elasticity produced by it in the æther. Now, as it is certain that the arrangement of the molecules of the crystal in the particular shape gave rise to the axes of elasticity in the æther, we are shown a body whose shape produces a certain result, which result bears no relation to the shape that produced it. If the imprisonment of the æther between the molecules of a body produces axes of elasticity in that æther, then surely these axes must bear some close geometrical relation to the shape in which the molecules are arranged.

3. *Optical Properties of Carbon Compounds.*

The researches of Gladstone and Dale, of Landolt, and, more recently, of Brühl, have opened up a field of inquiry which cannot but be productive of important results. The influences exerted by organic compounds upon light which passes through them are of two kinds, viz. (1) simple refraction of a ray, and (2) the turning of the plane of polarization. Let us briefly examine these separately.

1. *Refraction of Organic Compounds.*—From a series of observations made by Dulong it has been shown that not only does the density of a body change with the temperature, but also that the refractive index changes. The nature of this change is such that the expression $\frac{n-1}{d}$, called the "specific refractive power," where n is the refractive index and d the density, is approximately constant for different temperatures. Now, if we take the molecular weight of the body and multiply it by this constant quantity, we obtain a number which is called the molecular refracting-power of the body. Since the molecular weight depends on the chemical constitution, it follows that the molecular refracting-power depends on the chemical constitution also. From numerous experiments it has been found that:—

(*a*) Isomerides and metamerides have the same molecular refracting-power.

(*b*) In a series of compounds which differ by H_2, the molecular refracting-power differs by 2·6.

(*c*) In homologous series the molecular refracting-power increases 7·6.

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(*d*) The molecular refracting-power of the acids of the fatty series is $7{\cdot}6n+6$, where n is the number of atoms of carbon in the acid. But if the increase in molecular refracting-power for H_2 is equal to 2·6, that for H will equal 1·3. Thus we get a number which gives us the atomic refracting-power of one atom of H in these compounds. In the same way the atomic refracting-power of carbon is found to be 5, and that of oxygen 3. Knowing these numbers, the molecular refracting-power of a body of known composition can be calculated. For example, take ethylic acetate, $C_4H_8O_2$: by calculation we have $4\times5+8\times1{\cdot}3+2\times3=36{\cdot}4$ for the value of $\frac{n-1}{d}M$. By actual experiment d is found to be ·8977, $n=1{\cdot}3715$, and $M=88$; thus again giving 36·4. These results have been obtained from the researches of Gladstone and Dale and Landolt. Recently the subject has been taken up with marked success by Brühl (*Annalen*, vols. cc. and cciii., *Berichte*, vol. xiii.) He has corrected the values given above for the atomic refracting-powers of carbon, hydrogen, and oxygen, and has determined others. (Gladstone has also given a very complete list.) Brühl has deduced the following conclusions:—

(1) The atomic refraction of monad elements is constant.

(2) When an organic body loses two atoms of H, forming a compound in which two of the carbon atoms are linked together by a double affinity, the specific gravity increases by ·04, the coefficient of refraction by ·02; and the dispersion is also greater.

(3) Variations in atomic refraction occur in the case of multivalent elements.

Now here we have a mass of facts deduced from a large number of experiments plainly showing the very close relation that exists between the atomic structure of pure chemical compounds and the transmission of light through them. The relationship is in fact so close that the deduction even forces itself upon us, that the molecules of bodies are themselves the vehicles of light-motion. Assuming the existence of the luminiferous æther between the molecules of bodies, and also assuming that the wave-motion is transmitted by the æther, how are these results explained? Examined in whatever way we please, they will still remain inexplicable. Let us consider (as an example of the whole) the first of the rules, viz. isomeric and metameric bodies have the same refractive power. As isomeric and metameric bodies have the same percentage composition and the same number of atoms of each element, if we consider the molecules of bodies themselves concerned in the

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transmission of light through them, then it is a natural result that the refractive powers of such bodies should be the same. But if, on the contrary, we consider the æther which fills the spaces between these molecules as the medium which transmits the light-waves, then there is no simple connexion between the two. We have, if we wish to establish any connexion whatever, to make two additional assumptions, which it is easy to show are not justified by facts. These assumptions are, first, that the spaces between the molecules of these bodies are of equal size; and, secondly, that the shape of the spaces is the same in all bodies, or else the freedom of the æther filling these spaces to vibrate would be different. It is evident that these assumptions are necessary; for if in one body the spaces filled by the æther are larger than in another, then we should have that the freedom of the æther to vibrate is greater in one case than in another, and therefore that the refractive powers are different. Now we know from experiments upon isomeric and metameric bodies, that the specific gravity of these bodies is not constant. It varies considerably in some cases. For example, the specific gravity of allylic alcohol is ·8709 at 0° C., and that of acetone, isomeric with it, is ·792. But since the molecules are made up of the same chemical elements united in the same proportions, these molecules have the same density. The difference in density must therefore arise from a difference in the mean distances apart of these molecules; and thus the spaces enclosed by the molecules must be of different size. Again, are the spaces of the same shape? The constitutional formulæ of chemical compounds are deduced from the way these compounds behave when treated with chemical agents. Although no chemist would be found to say that it was a matter of absolute truth that the atoms were arranged in the molecule in the manner shown by its constitutional formula, yet in very many organic compounds the abundance of accumulated evidence is so vast that it is a matter of great probability that such is the case. Thus we gain an insight into the shape of the molecules imbedded in the æther, and therefore also of the shape of the spaces between these molecules. The constitutional formulæ of isomeric and metameric bodies are, as is well known, very various. In fact, isomeric bodies are distinguished in chemical formulæ by the difference in their constitutional formulæ. There can be no doubt, therefore, that the spaces between the molecules are *not* of the same shape; and thus the assumption is incorrect. The investigation of the first law of Gladstone and Dale thus reveals insuperable difficulties in the assumption of the intermolecular existence of the æther. All the remaining laws are

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equally difficult of explanation on the æther theory, but are natural consequences of the molecular theory. All show either that the addition of more atoms to the molecule or the arrangement of the atoms in different ways causes a difference in the ability of those molecules to oscillate, and therefore causes a difference in the refractive powers.

2. *Colour of Organic Compounds.*—Most organic compounds are colourless, as is well known; but those which are coloured are neither few nor unimportant. The connexion between the chemical constitution and the colour of the carbon-compounds has not been well worked out. It is, however, a most remarkable fact that most, if not all, of these coloured bodies contain certain elements (as, for example, nitrogen and oxygen) united in a peculiar manner. Now that there should be the remotest connexion between the colour of a body and the atoms of which that body is made up is a circumstance that cannot be explained upon the æther theory. The presence of the atoms is only important on this theory in so far as they affect the æther which occupies the spaces between them. Why the presence of particular atoms should affect this æther more than others is not clear. But if we assume that the molecules take up and transmit the vibrations of the light-waves, then it is evident that these molecules must be made different by the presence of different elements; and this difference gives rise to the colours of the substances.

3. *Circular Polarization.*—The relation between the chemical composition of substances and their rotary power has recently been studied by Landolt. In his paper upon the subject (Liebig's *Annalen*, clxxxix. pp. 241–337) he divides active bodies into three classes:—

(1) Those active only when in the crystalline form.
(2) Those active only when in solution or when fused.
(3) Those active when crystalline as well as when in solution.

The bodies belonging to the first class we have already considered. Only two bodies are known belonging to the third class. The second is therefore the most important. Concerning the second class, Landolt considers it proved beyond doubt that the activity of these bodies is a property of the molecules, and depends on the arrangement of the atoms in those molecules. In support of this latter statement he quotes the experiments of Biot on camphor and turpentine-oil (*Mémoires de l'Acad.* vol. ii. p. 114). In fact, Biot has applied to the rotation produced by these bodies the term "*molecular* rotation," to distinguish it from crystalline rotation. Again, Van t'Hoff has found, from a comparison of all known active

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and many inactive bodies, that all the former possess one or more asymmetrical carbon-atoms—an asymmetrical carbon-atom being one which is united with unlike simple or compound radicals. Here, then, we have a mass of evidence which unquestionably shows that optical rotation depends upon the *molecules* of which the bodies are built up. But this rotation is produced by the passage of a ray of light through the body. Therefore the passage of a ray of light through a body depends upon, and is affected by, the structure of the molecules of that body. Hence the molecules of the body are the vehicles which transmit the light. Assuming the intermolecular existence of the luminiferous æther, these experiments upon optical rotation cannot be explained without making assumptions which are unreasonable. If this æther does transmit the vibrations of light, then the above conclusions regarding the molecules and atoms are not just, because we now have to deal, not with the molecules, but with the spaces between these molecules. It would thus appear that the study of optical rotation offers very strong evidence in favour of the molecular theory of vibration, while it throws great difficulty in the acceptance of the æther theory.

4. *The Photophone.*

The difficulty in explaining the decrease in the electrical resistance of selenium on exposure to light was briefly noticed in my previous paper. Since that time, however, the brilliant success which has attended the experiments of Prof. Graham Bell and Mr. Sumner Tainter has given the subject fresh interest. From being an interesting scientific curiosity, these researches have shown it to be but a result of a general law. It will therefore be of advantage to notice briefly the chief results which the various investigators have obtained in this particular subject.

In addition to the discovery of the fundamental fact, Mr. Willoughby Smith* proved that solar light has a much more powerful effect upon selenium than light from any other source. Lieutenant Sale†, who was the next investigator to publish his results, came to the conclusion that the effect is not due to any change in the temperature of the selenium bar, and also that the effect is produced instantaneously. He concluded that there exists a power in rays nearly coincident with the heat-rays of low refrangibility "of altering instantaneously the

* Journal of the Society of Telegraph Engineers, vol. vi.
† Proc. Roy. Soc. vol. xxi. p. 283.

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molecular condition of the substance selenium." Lord Rosse* instituted some experiments to determine whether the effect is to be attributed to heat or to light. He concludes that the effect is due to the action of light and not to that of heat, and that the variation of the resistance is proportional rather to the square root of the intensity of illumination than to the intensity simply; but no definite law of this kind could be deduced. Dr. Werner Siemens confirmed Lord Rosse's results, and, in addition, proved that the electrical properties are modified by the temperature at which the selenium has been produced†. His experiments lead him to the conclusion that the effect of the light upon the selenium is to produce a change in the molecular condition of the substance near its surface, by which it is more or less converted from an electrolytic conductor into a metallic conductor, involving a liberation of specific heat upon that surface which is exposed to the light-rays. The exhaustive experiments of Prof. W. G. Adams and Mr. R. E. Day led the former to propound two possible hypotheses by which the action of light on selenium may be explained:—(1) that the light falling on the selenium causes an electromotive force in it, which opposes a battery-current passing through it, the effect being similar to the effect due to polarization in an electrolyte; and (2) that the effect of light is to cause a change on the surface analogous to the change which it produces on the surface of a phosphorescent body, and that in consequence of this change the electric current is enabled to pass more readily over the surface of the selenium‡. These experimenters proved, among other things, that the effect was directly proportional to the square root of the intensity of the illumination, and also that an electric current was produced in a piece of crystalline selenium when suddenly exposed to light. One of the conclusions to which they were led is significant. They consider that the first current sent through the selenium causes a more or less permanent "set" of the molecules, in consequence of which the passage of the succeeding currents is more resisted in that direction than it is in the opposite one. They also proved that this effect was considerably modified if the bar was exposed to light, thus conclusively showing that the waves of light impinging on the material molecules of the selenium affected those molecules in in such a manner as to cause them to be less affected by the passage of an electric current through them. The subject

* Phil. Mag. vol. xlvii. No. 311.

† *Monatsbericht der Akademie der Wissenschaften zu Berlin*, 1875, p. 80, and January 1876.

‡ Proc. Roy. Soc. vol. xxiii. p. 535.

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was in this condition when it was taken up by Graham Bell and Sumner Tainter. These gentlemen made a departure from the mode of experimenting adopted by all previous investigators in that they employed the telephone instead of the galvanometer as an indicator of the existence of the electric currents. This change was of advantage from a qualitative point of view, but not from a quantitative one. We consequently find that the advances which these gentlemen have made are of the nature of advancing the bounds of the subject rather than of giving us a more exact knowledge of the facts we already possess. Their chief results, considered theoretically, are:—

1st. That the molecular vibrations of a mirror are able to affect light-waves which fall upon it, in such a manner that when these waves fall upon a second mirror they are able to reproduce in that second mirror the vibrations of the first.

2nd. That several bodies are capable of transmuting luminous undulations into sonorous ones.

We may take this rapid sketch as giving the chief results which have been obtained up to the present time. I have omitted from consideration the question whether the effects are due to heat or to light, because this is a point which is not of any importance to us. The difference between the waves of radiant heat and those of light is a difference which is one of degree only and not of kind. A particle of matter oscillating four hundred billion times a second will produce a wave of non-luminous heat; while if the same particle be caused to oscillate six hundred billion times a second, it will emit a wave of light. It is therefore in the highest degree probable that the self-same medium takes up and transmits the undulations of both radiant heat and light; in fact the supporters of the æther theory themselves make this assumption. Now, looking at the results of all these investigators, from Willoughby Smith to Graham Bell, it is utterly impossible to avoid coming to the conclusion, *that the waves which, falling on the retina, produce the sensation of light, also produce vibrations in the molecules of other bodies upon which they impinge*; and also that *the vibrations of molecules of bodies affect the luminous undulations which fall upon them.* Here we have results which are obvious deductions from the theory of the molecular transmission of light-waves which I have endeavoured to establish, but which are utterly inexplicable on the æther theory. Why should the vibrations of the elastic æther affect the molecules of bodies in this particular manner? and, conversely, why should the oscillations of the molecules affect the vibrations of the æther? The experiments plainly show that there is no breach

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in continuity from the comparatively gross oscillations which produce sound up to those which give rise to radiant heat and light. Why, then, should our æther theory produce a break? Yet this is what the æther theory does, because all suppose that sound is transmitted by the vibrations of the molecules of matter itself. If, then, we imagine heat and light to be transmitted by the undulations of particles of æther, at some point which we do not know we must have that the transmission of the vibratory movement by matter ceases, and its propagation by æther begins. It is unnecessary to say that there is no experimental evidence whatever of the existence of this point. The close relationship between the waves producing radiant heat and those producing sound is also fully shown in a communication recently made to the Royal Society by Prof. Tyndall*. In this paper it is shown that, like solids, gases and vapours are capable of producing the degradation of waves of light and heat, and converting them into sound†. This view of the phenomenon is also held by Prof. Tyndall, who speaks of the rays transmitted by iodine as being "converted into sound."

In the foregoing pages we have considered some of the deductions to be drawn from recent researches. These deductions, one and all, lend support to the molecular theory, while they give to the æther theory a great degree of improbability, this improbability in some cases amounting to almost an absolute proof of its non-existence. Into the much discussed question of action at a distance it is not necessary to enter. Whether action at a distance does or does not take place, so far as our present purpose is concerned, is beside the question. All that it has been the object of the author to show is, that the phenomena of light can be explained simply and completely without calling into existence a hypothetical substance.

In conclusion, the chief results of this inquiry are briefly stated as follows:—

(α) The action of the radiometer, depending, as it does, upon the movements of the residual gas, is unaffected by the presence of the æther.

(β) The action of heat upon crystalline bodies, whereby their optical properties are changed, is difficult of explanation, unless we assume that the same molecules are concerned in the oscillations which produce heat and also those which propagate light.

* "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." Read before the Royal Society, Jan. 13, 1881.

† May not the term "sonorescence" be applied to this phenomenon? See paper in the May Number of this Journal.

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(γ) The refraction-equivalents of organic compounds so closely depending on the chemical constitution of those bodies shows the very close relation which exists between the particles which transmit light and the elements of the body.

(δ) The study of optical rotation affords an insight into the close relationship between matter and light.

(ϵ) The action of light in altering the electrical resistance of bodies and also in producing sound shows that there is no break in continuity between sound and light.

LXV. *On the Beats of Consonances of the Form* h : 1. *By* R. H. M. Bosanquet.

[Concluded from p. 435.]

Combination-Tones arising from Terms of Orders higher than the first, in the Transforming-structure of the Ear.

57. Helmholtz pointed out the way in which the hypothesis of asymmetry in the transmitting mechanism of the ear gives rise to the combination-tone of the first order, which he called the difference-tone. This asymmetry was represented in his investigation by a term of the second order in the force called into play by a given displacement. Helmholtz further indicated the tones to which the existence of terms of the third order gives rise, and pointed out that tones of the fourth order &c. would give rise to other combination-tones not further specified.

58. In specifying those combination-tones which arise from the terms of the third order, Helmholtz pointed out that one of them was a combination-tone of the second order "in the sense indicated by Hallström." This is also the sense in which the expression "combination-tone of the second order" is used by Helmholtz himself in the text of his work; and it means apparently a combination-tone which arises from a combination-tone or combination-tones of the first order when combined with any other notes present, or with each other, according to the law of combination-tones of the first order.

59. It is clear, however, that the principal combination-tones which arise from the terms of higher orders in the transmitting mechanism of the ear, are derived directly from the primary tones, and are not materially influenced by the secondary series of tones. This is obvious, on the one hand, since all the resultant tones, according to the principles of their origin, are of the nature of small quantities compared with the primary tones, and, on the other hand, because the tones derived from the terms of higher orders are in fact produced

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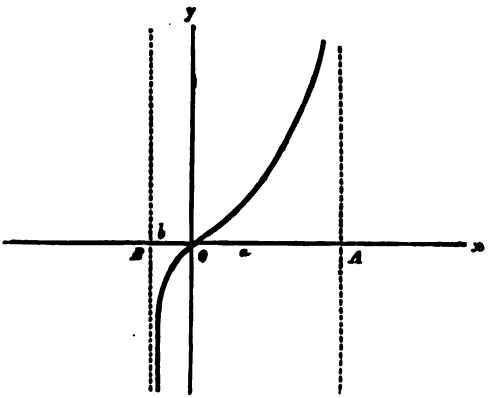
with the greatest intensity when the tones derived from terms of lower orders are weak or evanescent. This fact has been used by König as one of the most powerful objections to the theory of combination-tones as hitherto expounded; and, indeed, the objectionable character of some of the hypothetical derivations by combination given by Helmholtz* must have struck many readers independently.

60. I shall now examine Helmholtz's hypothesis of asymmetry in a little more detail; and I think it will appear that it leads, by tolerably simple mathematical treatment, to the development of the combination-tones of the higher orders, under the circumstances under which they actually exist, and independently of the combination-tones of the lower orders.



61. Let 0 represent the position of rest of a point free to move along the line $0x$ between the points A and B, subject to certain forces in that line. Suppose that these forces are of the nature of springs tending to resist the departure from 0, and that on arrival at the points A, B, at distances a, b, from 0 on either side, the springs ultimately go up against dead walls, so that further displacement is resisted with an infinite force. If we set off the forces as ordinates, they may be represented as in the figure; and analytically they may be expressed by such an assumption as

$$y = \frac{kx}{\left(1-\frac{x}{a}\right)\left(1+\frac{x}{b}\right)}.$$

Expanding this function in a series proceeding by powers of

* *Tonempfindungen*, 4th ed. p. 329; also p. 327, where the combinations are supposed to be formed with the partials of the primaries as well.

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x, we have an example of a law of force expressed by such a series, which for small displacements coincides with the pendulum law, while for displacements of but moderate extent the higher terms rapidly become prominent.

62. Assuming the existence of a law of this kind in the transmitting mechanism of the ear, we should have for the force corresponding to displacement u such an expression as

$$-(n^2u+\alpha u^2+\beta u^3+\gamma u^4+\dots).$$

Suppose the system acted upon by two harmonic forces,

$$\mathrm{E}\cos pt, \quad \mathrm{F}\cos(qt-\epsilon),$$

and the mass = unity, or included in the coefficients; the equation of motion is

$$\frac{d^2u}{dt^2}+n^2u=-\alpha u^2-\beta u^3-\gamma u^4\dots+\mathrm{E}\cos pt\,\mathrm{F}\cos(qt-\epsilon)$$

(following the notation of Lord Rayleigh on Sound, i. p. 65).

For the first approximation we neglect powers of u above the first; then

$$u=e\cos pt+f\cos(qt-\epsilon),$$

where

$$e=\frac{\mathrm{E}}{n^2-p^2}, \quad f=\frac{\mathrm{F}}{n^2-q^2}.$$

63. We may here mention that, in the present case, n is negligible. This is easily seen, since, if n had any value corresponding to a frequency within the limits of the ordinary range of hearing, there would be a series of notes strengthened by the correspondence. But the only notes thus strengthened are those which are supposed to correspond to the ear-cavity. They are so high in the scale that the connexions of the internal ear would require to be nearly as rigid as brass or steel to produce them. A further reason for n not being large will be arrived at in speaking of combination-tones. And we shall assume that n is smaller than any values of p or q which occur in practice.

64. The first approximations to the subsequent terms may be now all made by substituting in them the value of u above obtained. The process to be followed for u^2 coincides with that commonly adopted; and the result is given in Lord Rayleigh's book, i. p. 66.

In the cases of u^3 and higher powers the process is simpler than that which has been previously indicated.

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65. In virtue, however, of the preceding considerations concerning the value of n, we may materially simplify the whole process for our present purpose by neglecting the term n^2u altogether. The original equation then assumes the form

$$\frac{d^2u}{dt^2} = -(\alpha u^2 + \beta u^3 + \ldots) + \mathrm{E}\cos pt + \mathrm{F}\cos(qt-\epsilon).$$

First approximation,

$$\begin{aligned} u &= \mathrm{D}^{-2}\left(\mathrm{E}\cos pt + \mathrm{F}\cos(qt-\epsilon)\right) \\ &= -\left(\frac{\mathrm{E}}{p^2}\cos pt + \frac{\mathrm{F}}{q^2}\cos(qt-\epsilon)\right) \\ &= -\left(e\cos pt + f\cos(qt-\epsilon)\right) \text{ say.} \end{aligned}$$

Substituting this in the remaining terms, we get

$$\begin{aligned} \frac{d^2u}{dt^2} = {} & \mathrm{E}\cos pt + \mathrm{F}\cos(qt-\epsilon) \\ & -\alpha\left(e^2\cos^2 pt + f^2\cos^2(qt-\epsilon) + 2ef\cos pt\cos(qt-\epsilon)\right) \\ & -\beta\left(e^3\cos pt + f^3\cos^3(qt-\epsilon) + 3e^2f\cos^2 pt\cos(qt-\epsilon)\right. \\ & \qquad \left. + 3ef^2\cos pt\cos^2(qt-\epsilon)\right) \\ & -\gamma\left(e^4\cos^4 pt + f^4\cos^4(qt-\epsilon) + 4e^3f\cos^3 pt\cos(qt-\epsilon)\right. \\ & \qquad \left. + 6e^2f^2\cos^2 pt\cos(qt-\epsilon) + 4ef^3\cos pt\cos(qt-\epsilon)^3\right) \\ & - \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad ; \end{aligned}$$

and u is, to this first approximation with respect to all terms, the integral taken twice with respect to t of the right-hand side of the above equation.

66. There is, no doubt, a difficulty as to the absolute neglect of the term n^2u. The effect is to make the vibrating-point apparently rest in a position which is not one of equilibrium. Nevertheless the application of the facts to Helmholtz's hypothesis requires this proceeding; and it makes no difference whether it is done finally or at first. I think it very probable that damping terms, depending on the second and higher powers of the velocity, play an important part in the real explanation. The source of the terms, however, is of secondary importance in the present state of the question. The point is to show that those resultant sounds which depend on terms of higher orders can become great independently of those which depend on terms of lower orders.

67. Collecting the terms up to the fourth order, transform-

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ing them into multiple arcs, and writing $pt=\theta$, $qt-\epsilon=\phi$, the equation becomes

$$\begin{aligned}\frac{d^2u}{dt^2} &= \mathrm{E}\cos\theta+\mathrm{F}\cos\phi \\ &-\Big[\frac{\alpha}{2}(e^2+f^2)+\tfrac{3}{8}\gamma(e^4+f^4+4e^2f^2) \\ &+\tfrac{3}{4}\beta\{(e^2+2f^2)e\cos\theta+(2e^2+f^2)f\cos\phi\} \\ &+\frac{e^2}{2}\{\alpha+\gamma(e^2+3f^2)\}\cos 2\theta+\frac{f^2}{2}\{\alpha+\gamma(3e^2+f^2)\}\cos 2\phi \\ &+\frac{\beta}{4}(e^3\cos 3\theta+f^3\cos 3\phi) \\ &+\frac{\gamma}{8}(e^4\cos 4\theta+f^4\cos 4\phi) \\ &+ef\{\alpha+\tfrac{3}{2}\gamma(e^2+f^2)\}(\cos(\theta+\phi)+\cos(\theta-\phi)) \\ &+\tfrac{3}{4}\beta e^2f\{\cos(2\theta+\phi)+\cos(2\theta-\phi)\} \\ &+\tfrac{3}{4}\beta ef^2\{\cos(\theta+2\phi)+\cos(\theta-2\phi)\} \\ &+\tfrac{3}{4}\gamma e^2f^2\{\cos 2(\theta+\phi)+\cos 2(\theta-\phi)\} \\ &+\gamma\frac{e^3f}{2}\{\cos(3\theta+\phi)+\cos(3\theta-\phi)\} \\ &+\gamma\frac{ef^3}{2}\{\cos(\theta+3\phi)+\cos(\theta-3\phi)\}\Big].\end{aligned}$$

68. On performing the double integration, we shall find the constant term in the above multiplied by t^2, an inadmissible result. It is only necessary to look back to the result of the complete process, when we find that the term in question is represented after integration by $\frac{\text{constant}}{n^2}$, where n^2 is the small coefficient of the term we have neglected. This indicates that the position of equilibrium is indeed displaced, but through a finite amount; as this does not affect our results, we omit the term in question.

69. Remembering that $\theta=pt$ and $\phi=qt-\epsilon$,

$$e=\frac{\mathrm{E}}{p^2},\quad f=\frac{\mathrm{F}}{q^2},$$

the remainder of the equation becomes

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$$u = -e\cos\theta - f\cos\phi$$
$$+\tfrac{3}{4}\beta\left\{\frac{e^2+2f^2}{p^2}e\cos\theta + \frac{2e^2+f^2}{q^2}f\cos\phi\right\}$$
$$+\frac{e^2}{2}\frac{\alpha+\gamma(e^2+3f^2)}{4p^2}\cos 2\theta + \frac{f^2}{2}\frac{\alpha+\gamma(3e^2+f^2)}{4q^2}\cos 2\phi$$
$$+\frac{\beta}{4}\left(\frac{e^3}{9p^2}\cos 3\theta + \frac{f^3}{9q^2}\cos 3\phi\right)$$
$$+\frac{\gamma}{8}\left(\frac{e^4}{16p^2}\cos 4\theta + \frac{f^4}{16q^2}\cos 4\phi\right)$$
$$+ef\left\{\alpha+\tfrac{3}{2}\gamma(e^2+f^2)\right\}\left(\frac{\cos(\theta+\phi)}{(p+q)^2}+\frac{\cos(\theta-\phi)}{(p-q)^2}\right)$$
$$+\tfrac{3}{4}\beta e^2 f\left\{\frac{\cos(2\theta+\phi)}{(2p+q)^2}+\frac{\cos(2\theta-\phi)}{(2p-q)^2}\right\}$$
$$+\tfrac{3}{4}\beta ef^2\left\{\frac{\cos(\theta+2\phi)}{(p+2q)^2}+\frac{\cos(\theta-2\phi)}{(p-2q)^2}\right\}$$
$$+\tfrac{3}{4}\gamma e^2 f^2\left\{\frac{\cos 2(\theta+\phi)}{4(p+q)^2}+\frac{\cos 2(\theta-\phi)}{4(p-q)^2}\right\}$$
$$+\gamma\frac{e^3 f}{2}\left\{\frac{\cos(3\theta+\phi)}{(3p+q)^2}+\frac{\cos(3\theta-\phi)}{(3p-q)^2}\right\}$$
$$+\gamma\frac{ef^3}{2}\left\{\frac{\cos(\theta+3\phi)}{(p+3q)^2}+\frac{\cos(\theta-3\phi)}{(p-3q)^2}\right\};$$

so that there are six summation-tones and six difference-tones produced by direct transformation of the primaries, when the effect of terms up to the fourth order is considered.

70. The effect of the neglect of n^2 in the denominators of all these terms, is to place the principal development of any term such as the difference-tone $p-q$ at the point where $p-q=0$, whereas if the complete solution were retained the condition for the principal development would be

$$n^2-(p-q)^2=0.$$

No known phenomenon enables us to distinguish between these two cases. Every thing happens, so far as we know, precisely as if the simpler condition were that which is really important.

71. If we proceed to terms of higher orders in the same way, we shall always have, in the result of terms of the $\overline{n+1}$th order, the two following terms representing nth difference-tones, which alone are important for our present purpose

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(a_{n+1} is the $\overline{n+1}$th coefficient),

$$\frac{(n+1)a_{n+1}}{2^n}\left\{\frac{e^n f\cos(n\theta-\phi)}{(np-q)^2}+\frac{ef^n\cos(\theta-n\phi)}{(p-nq)^2}\right\},$$

besides other terms analogous to those shown above.

72. In the neighbourhood of any consonance of the form $h:1$, the terms having the denominators $(hp-q)^2$ become large; this is Helmholtz's explanation of the origin of difference-tones, generalized.

73. As the argument from the analytical expressions fails to give perfect satisfaction unless the nature of the causes involved be more directly demonstrated, I shall try to show more simply how it is that this comes about.

In periodic functions such as $\cos pt$, $\cos(qt-\epsilon)$, the quantities p, q are such that, if τ, τ' be the periodic times,

$$p\tau=q\tau'=2\pi, \text{ or } p=\frac{2\pi}{\tau}, \quad q=\frac{2\pi}{\tau'}.$$

If, then,

$$M\tau=1,$$
$$N\tau'=1,$$

M, N are the frequencies of the primaries, and

$$p=2\pi M, \quad q=2\pi N.$$

In the case of a mistuned consonance of the form $h:1$, the denominator of the h difference-tone term in the above expression will be $4\pi^2(hM-N)^2$. And $hM-N$ is the frequency of the beat which gives rise to the transformation according to all theories (putting $k=1$ in the more general formula $hM-kN$).

$\therefore \frac{1}{hM-N}$ is the time of duration of the beat of the resultant form, whether we call it the Smith's beat, or the bow of the pendulum curves. As the denominator diminishes, the time or duration of the beat increases.

74. What happens, then, is that a force is developed, by the influence of the higher terms in question, which acts for a time corresponding to the duration of the beat. And it is matter of ordinary mechanical knowledge that, under these circumstances, the space traversed is proportional to the square of the time during which the action lasts; so that when the beat is lengthened the effect of the transformation is strengthened.

75. It is possible to found an independent treatment of the subject on these considerations, the course of which would be somewhat as follows.

In mistuned consonances of the form $h:1$ there are alter-

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nate increases and diminutions of the maximum resultant displacement, the duration of which can be arrived at by the considerations employed by Smith in determining the duration of beats. The duration of one such increase and diminution can be shown by the known formula to be $\frac{1}{hM-N}$.

Assume that the transmitting mechanism of the ear possesses such powers of transformation that any regular sequence of increases and diminutions of maximum resultant displacement is capable of giving rise, by transformation, to a subjective note having the same period as that of one increase and diminution. This assumption only differs from that made above in definiteness of form; for the algebraic series which is above proved to give rise to transformations of this description, is itself an assumption.

It immediately follows, by considerations differing little from those made use of in the ordinary investigation of the motion of a particle under the action of a uniform force, that the coefficient of the term in question will contain the square of the periodic time—that is to say, the coefficient $\frac{1}{(hM-N)^2}$; and this is the essential point proved by the more complete analytical investigation above given.

76. Though perhaps defective as a complete demonstration of the *rationale* of the origin of difference-tones, these considerations render the general meaning of the coefficients of the difference-tone terms in the above equations tolerably clear. And we have thus sketched a method, in which the doctrine of transformation arising out of the Smith's beats, as the resultant forms pass through the transmitting mechanism of the ear, forms the basis for the further explanation of the phenomena of beats as we find them.

The Resultant Wave-forms of Mistuned Consonances.

77. I am principally acquainted with these forms as drawn by means of Donkin's harmonograph. The curves (Plates IV.–VII.) that accompany this paper exhibit all the points on which it will be necessary to touch.

78. It is hardly possible to be acquainted with these curves without seeing that the figures formed by the vertices which occur in the curves are in some way related to the phenomena of the mistuned consonances. And as I had myself considerable difficulty in coming to definite conclusions as to the real nature of this relation, and do not know of any published discussion of the subject, I add this article dealing with the rela-

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tion in question so far as it is connected with the subject of the paper.

79. The curves are referred to an axis of x, along which the wave-lengths are measured, and an axis of y parallel to which the displacements are measured. λ and λ' are the wave-lengths on the paper of the two primary curves. If it is required to consider a question of frequency, the paper must be supposed to be drawn past the observer with velocity v, when the frequencies will be $\frac{v}{\lambda}, \frac{v}{\lambda'}$ respectively.

80. The tangent of the inclination of a curve to the x-axis will be spoken of shortly as the "slope."

It is assumed that $q\lambda' = p\lambda + \delta$, where p, q are integers, $q > p$, and δ is small.

81. The equation of the resultant of two primary curves may then be written

$$y = \mathrm{E}\cos 2\pi\frac{x}{\lambda} + \mathrm{F}\cos\frac{2\pi}{\lambda'}(x-\alpha).$$

The slopes of the two single curves are

$$-\frac{2\pi}{\lambda}\mathrm{E}\sin 2\pi\frac{x}{\lambda}, \quad -\frac{2\pi}{\lambda'}\mathrm{F}\sin\frac{2\pi}{\lambda'}(x-\alpha).$$

The ratio of the coefficients is

$$\frac{\lambda'\mathrm{E}}{\lambda\mathrm{F}} = \frac{p\mathrm{E}}{q\mathrm{F}} \text{ nearly.}$$

When this ratio is much greater than unity, the resultant slope is nearly that of the first term. When it is much less than unity, the resultant slope is nearly that of the last term.

82. The general expression for the resultant slope is given by

$$\frac{dy}{dx} = -\frac{2\pi}{\lambda}\mathrm{E}\sin 2\pi\frac{x}{\lambda} - \frac{2\pi}{\lambda'}\mathrm{F}\sin\frac{2\pi}{\lambda'}(x-\alpha).$$

The vertices of the resultant curve are obtained by equating $\frac{dy}{dx}$ to zero, whence

$$\frac{\mathrm{E}}{\lambda}\sin\frac{2\pi x}{\lambda} + \frac{\mathrm{F}}{\lambda'}\sin\frac{2\pi}{\lambda'}(x-\alpha) = 0.$$

83. Case I., where F is great, and the first term negligible compared with the second ($\frac{\lambda'\mathrm{E}}{\lambda\mathrm{F}}$ small).

Here the vertices are those of the second component of the curve. Consequently, in every cycle of p and q vibrations of λ and λ' respectively, the q vertices of λ' appear, those of λ

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being smoothed out. The sole effect of the term involving λ is in this case to modify slightly the positions of the vertices.

84. If, then, $p\lambda$ were exactly $=q\lambda'$, then after a certain distance, which may be called a short cycle, the vertices would recur for precisely the same values of y. And the corresponding vertices in successive short cycles would lie on q straight lines, or on $2q$ straight lines if the lower vertices be included.

This short cycle is obviously $p\lambda=q\lambda'$ in duration.

85. Since, however, in our general case $p\lambda+\delta=q\lambda'$, the coincidence after the short cycle is not exact; but the vertex determined by equating the second term of the inclination to zero has, in the first term, a different correction to the value of y from that which existed before the short cycle.

86. At the vertex before the short cycle let $x=\alpha$, so that the second term of the inclination vanishes; then, before the short cycle,

$$y_0=\mathrm{E}\cos\frac{2\pi\alpha}{\lambda}+\mathrm{F};$$

after one short cycle, $x=q\lambda'+\alpha=p\lambda+\delta+\alpha$,

$$y_1=\mathrm{E}\cos\frac{2\pi}{\lambda}(\alpha+\delta)+\mathrm{F};$$

after two short cycles, $w=2q\lambda'+\alpha=2(p\lambda+\delta)+\alpha$,

$$y_2=\mathrm{E}\cos\frac{2\pi}{\lambda}(\alpha+2\delta)+\mathrm{F};$$

and so on, till after n short cycles, where $n\delta=\lambda$, nearly or exactly,

$$y_n=\mathrm{E}\cos\frac{2\pi\alpha}{\lambda}+\mathrm{F};$$

and the ordinate of the vertex in question has gone through a complete period of a pendulum-curve in the space

$$\begin{aligned} nq\lambda' &= n(p\lambda+\delta) \\ &= n\lambda\left(p+\frac{1}{n}\right), \text{ since } n\delta=\lambda \\ &= \lambda(np+1). \end{aligned}$$

87. Now we have seen that there are q of these vertices, each of which gives rise to one of these curves. Consequently this space, $(np+1)\lambda=nq\lambda'$, presents, both above and below, q projecting bows, and each bow is of length

$$\frac{np+1}{q}\lambda \text{ or } n\lambda'.$$

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This is the length of Smith's beat, or of the beat as given by a well-known formula. This is easily verified as follows:—

88. Let v be the velocity of sound corresponding to wave-lengths λ and λ', and M, N the corresponding frequencies; then

$$\frac{v}{\lambda}=\mathrm{M}, \quad \frac{v}{\lambda'}=\mathrm{N},$$

and $q\lambda'=p\lambda+\delta$ becomes $(n\delta=\lambda)$,

$$\frac{q}{\mathrm{N}}=\frac{p}{\mathrm{M}}+\frac{1}{n\mathrm{M}};$$

$$\therefore\ p\mathrm{N}-q\mathrm{M}=\frac{\mathrm{N}}{n}=\frac{v}{n\lambda'};$$

which connects the expression above obtained with the ordinary formula for the frequency of the beat. Hence the Smith's beat in this case corresponds in period to the projecting bow formed by the $\frac{1}{q}$th part of the whole periodic curve of slow disturbance of one of the vertices.

89. Case II., where $\frac{\mathrm{E}}{\lambda}=\frac{\mathrm{F}}{\lambda'}$, so that the condition for a vertex reduces to

$$\sin\frac{2\pi x}{\lambda}+\sin\frac{2\pi}{\lambda'}(x-\alpha)=0.$$

This condition gives the following series of values:—

$$\frac{x}{\lambda}=\frac{-x+\alpha}{\lambda'}, \qquad x=\frac{\alpha\lambda}{\lambda+\lambda'},$$

$$=\frac{-x+\lambda'+\alpha}{\lambda'}, \qquad =\frac{(\alpha+\lambda')\lambda}{\lambda+\lambda'},$$

. . . .

$$=\frac{-x+\nu\lambda'+\alpha}{\lambda'}, \qquad =\frac{(\alpha+\nu\lambda')\lambda}{\lambda+\lambda'},$$

until

$$\nu\lambda'=\lambda+\lambda';$$

and if

$$q\lambda'=p\lambda,$$

$$\nu=\frac{p+q}{p}.$$

Where this is not a whole number, the condition will be

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$$\nu\lambda' = k(\lambda + \lambda'),$$

$$\nu = k \cdot \frac{p+q}{p};$$

and if $p : q$ is in its lowest terms,

$$k = p, \text{ and } \nu = p + q,$$

$p+q$ is therefore the number of independent vertices arising from these terms.

90. Another series of values satisfies the condition; these are as follows (since $\sin x = \sin(\pi - x)$ &c.):—

$$\frac{x}{\lambda} = \frac{1}{2} + \frac{x-\alpha}{\lambda'}, \qquad x = \frac{\lambda' - 2\alpha}{2(\lambda'-\lambda)} \cdot \lambda,$$

$$= \frac{3}{2} + \frac{x-\alpha}{\lambda'}, \qquad = \frac{3\lambda' - 2\alpha}{2(\lambda'-\lambda)} \cdot \lambda,$$

$$\cdots\cdots \qquad \cdots\cdots$$

$$= \frac{2\nu-1}{2} + \frac{x-\alpha}{\lambda'}, \qquad = \frac{(2\nu-1)\lambda' - 2\alpha}{2(\lambda'-\lambda)} \cdot \lambda,$$

until

$$(2\nu)\lambda' = 2(\lambda - \lambda'),$$

$$\nu = \frac{\lambda' - \lambda}{\lambda'},$$

$$= \frac{p-q}{p}.$$

And if this be not a whole number,

$$(2\nu)\lambda' = 2k(\lambda - \lambda'),$$

$$\nu = k\left(\frac{q}{p} - 1\right),$$

$$\nu = \frac{k}{p}(p-q);$$

and if $p : q$ is in its lowest terms,

$$k = p, \quad \nu = p - q, \text{ or } q - p, \text{ since } q > p;$$

$q-p$ is therefore the number of independent vertices arising from these terms.

91. The relation of these different sets of vertices may be otherwise exhibited by putting the expression for the inclination into the form

$$\sin\frac{2\pi x}{\lambda} + \sin\frac{2\pi}{\lambda'}(x-\alpha)$$

$$= 2\sin\pi\left\{x\left(\frac{1}{\lambda} + \frac{1}{\lambda'}\right) - \frac{\alpha}{\lambda'}\right\}\cos\pi\left\{x\left(\frac{1}{\lambda} - \frac{1}{\lambda'}\right) + \frac{\alpha}{\lambda'}\right\} = 0.$$

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The zero values of the sine give the $p+q$ vertices of the first set; and the values which make the cosine vanish give the $q-p$ vertices of the other set.

92. Each of these vertices occupies, as in the former case, its special position in the short cycle $q\lambda'=p\lambda$, and lies always on a straight line $y =$ constant when such an exact relation holds.

93. Also, as before, when the above relation is changed into $q\lambda'=p\lambda+\delta$, it may be shown, by examining the successive arguments of the vertices, that they shift their position in successive short cycles, so that they lie on pendulum-curves of long period; also that the period of these curves is, for the $p+q$ system, $p+q$ times that of the Smith's beat, and for the $p-q$ system, $p-q$ times that of the Smith's beat.

94. The curves of both these systems, with the Smith's beats which form part of them, are readily recognized in all those of the pendulum-curve illustrations which approximately satisfy the condition

$$p\mathrm{E}=q\mathrm{F} \text{ or } \lambda'\mathrm{E}=\lambda\mathrm{F}.$$

In the case of the major third, where there are many vertices in each short cycle, and the figure of the short cycle is itself complicated, these curves are not easily recognized. It is necessary to mark a set of corresponding vertices in order to recognize the curve in this case. By the time we arrive at the fifth the curves are quite plain.

The curves of the $p+q$ system are large and bold, extending completely from top to bottom of the illustrations; each curve comprises the bow of a Smith's beat both above and below. These may be spoken of as the external system.

The curves of the $q-p$ system are smaller, and lie nearer the axial line of the illustrations. These may be spoken of as the internal system. In the particular case where $q-p=1$, such as $q=2$, $p=1$, the internal system exhibits complete periodic curves having the period of the Smith's beat.

95. Case III., where F is so small that $q\mathrm{F}$ is small compared with $p\mathrm{E}$. This would fall under the argument of the first case, with the signification of the letters reversed. But as we made the convention $q>p$, there arise some special points of difference.

96. Where q is much greater than p, as is the case of high harmonics combined with a fundamental, F has to be very small indeed in order that $q\mathrm{F}$ may be small compared with $p\mathrm{E}$. In this case, unless F is almost evanescent, it is not generally true that the only vertices are those of E (the fundamental); for in these cases the vertex of the fundamental curve becomes

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almost a straight line in the short space occupied by a wave of the higher curve; and under these circumstances the vertices of the higher curve continue to be visible wherever they come upon the vertex of the lower, especially where the two vertices are turned opposite ways.

The conditions of case I. not being strictly fulfilled, the consequences there deduced do not strictly follow. The considerations as to the number of different vertices which develop curves are not materially affected. And it remains true that there are always p curves (in case I. q curves) actually developed; but it is not true that there are no traces of any of the other q curves of the entire external set of $p+q$ of case II. On the contrary, it is seen in several of the illustrations, where for the most part $p=1$, that, instead of the outline being one pendulum-curve embracing the outlines of all the Smith's beats, the internal vertices of the long curves present traces of the crossing of two pendulum-curves of longer period—an effect which is seen to survive from the more general cases, on comparing the illustrations to case II. As the amplitude of the higher note diminishes, this curve assumes a trochoidal form, the external vertices being less sharp than the internal, where there is the survival from the crossing. Ultimately, no doubt, the outline would become theoretically a pendulum-curve. But, in the case of indefinite diminution of the coefficient $\frac{q\mathrm{F}}{p\mathrm{E}}$, where $\frac{q}{p}$ is great, $\frac{\mathrm{F}}{\mathrm{E}}$ is of the order of the product of two small quantities; consequently the effects on the displacements, or the curves we are examining, would themselves tend to become evanescent before their peculiarities; consequently the curve enveloping the Smith's beats would never in this way be reduced to a pendulum-curve having the period of those beats.

In the application of these considerations we have, further, to remember that the resultant tones which present pendulum-curves having the periods of Smith's beats are only heard when both notes are pretty loud; and under these circumstances the indefinite diminution of the ratio above supposed is not admissible. The only case, therefore, in which a locus of vertices is a pendulum-curve of the same complete period as the period of Smith's beat, is that of an internal system under case II., where $q-p=1$. As the existence of this system depends on the accurate adjustment of the coefficients to the law $p\mathrm{E}=q\mathrm{F}$, it cannot be referred to even as an illustration of a phenomenon of general occurrence.

97. We conclude, in conformity with the explanation at the end of the former part, (1) that the forms exhibited by the

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resultant of two pendulum-curves do not, as a rule, exhibit any appearances corresponding to pendulum-curves having the period of the Smith's beat, except in a very small number of cases, the conditions for which can only be fulfilled by accident; (2) that the increases and diminutions of the maximum displacement which form what we have called the bows of the harmonic curves, correspond in duration with the Smith's beat, but not in the period of the harmonic curves of which they form part.

98. We infer from the previous parts of this paper:—(3) that the variations of maximum displacement which are represented in these figures by the bows of the harmonic curves, give rise by transformation to pendulum-vibrations having the same frequency as those variations—these being the notes which König calls beat-notes, and Helmholtz difference-tones of various orders; and (4) that the actual beats of mistuned consonances of the form $h : 1$, as heard by the ear, are given rise to by the interference of these beat-notes or difference-tones with the lower note of the combination.

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99. The upper numbers prefixed to the Plates of curves are the ratios of the wave-lengths; the lower ones the ratios of the amplitudes.

Typical curve of case I. . $\frac{27}{80}$
9 : 8

Typical curve of case II. . $\frac{27}{80}$
3 : 1

Typical curve of case III. $\frac{27}{80}$, or rather $\frac{1}{3} \times \frac{80}{79}$
9 : 2 10 : 1

Postscript.—The curves shown in the Plates are all illustrations of the subject of this paper, with the exception of three sets; namely, the combinations of vibrations whose wave-lengths are nearly as 4 : 5, as 2 : 3, and as 2 : 5. These have been given for the sake of completeness in the collection of curves, and that readers may have the opportunity of seeing the nature of the difference between such curves as these, which may be said to belong to mistuned consonances of the form $h : k$, and our normal forms which belong to mistuned consonances of the form $h : 1$.

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LXVI. *Note on Stereoscopic Vision.* By Professor HELMHOLTZ*.

THE motives by which we judge the distance of the objects before us with one eye are the following:—

1. The outlines of the more distant objects are covered by those of the nearer, where they meet. It is this circumstance which causes the difficulty we have in recognizing the fact that the image projected by a convex lens or a concave mirror is nearer to the observer than the lens or the mirror.

2. The object which throws a projected shadow upon any surface is situated always before that surface. These first two motives are very rarely overpowered by any other ones —as, for instance, by stereoscopic combination. This is easily demonstrated by Dove's pseudoscope, an instrument containing two rectangular prisms, and showing to each eye a reflected image inverted from right to left. It produces an inverted stereoscopic effect if there are no projected shadows, and no outline covered by the outline of a nearer body.

3. If the head is moved to the right or the left, upwards or downwards, the direction of the eyes remains steady if the object observed is infinitely distant, but is altered the more the nearer the object is. If the head is brought nearer to the object, the convergence of the eyes increases; if it is moved backwards, the convergence diminishes.

We may call the peculiarity of the eye producing these phenomena the *parallax for motion of the head*. At the same time the relative situation of objects of different distance in the field of vision is altered. Distant objects apparently go with the observer, nearer objects in opposite directions. Also this overpowers stereoscopic combination. In Brewster's stereoscope the two images are brought together with a moderate convergence of the eyes, but are nearly in the focus of the lenses, so that these project images at an infinite distance. The instrument is adapted to presbyopic vision. But if it is fastened to a table and the head of the observer is moved, the objects appear to be far more distant than the point of convergence.

I concluded from this and similar observations that the perception of the absolute convergence of our eyes is very indistinct, and that only differences of convergence, related to apparently near or distant objects, produce the stereoscopic effect. But lately I have observed that certain apparent motions of binocular objects may be observed, which prove that

* Communicated by the Physical Society, having been read at the Meeting on April 9.

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the incongruence between the degree of convergence and the parallax of motion is perceived with great accuracy.

The easiest way to see them is the following:—Look to a papered wall, the pattern of which is regularly repeated at distances not much greater than the distance between your eyes (between 60 and 70 millim.). You know that it is possible to make your eyes converge either to a nearer or to a more distant point than the surface of the paper; so that in your binocular field of vision two images get corresponding position, which do not belong to the same part of the paper, but to two different copies of the pattern. You see, then, a stereoscopic image of the pattern, either more distant and of greater apparent size, if you diverge your eyes, or nearer and smaller, if you converge. But the appreciation of the apparent distance of this pattern is not very precise. If you try to bring a pencil to the apparent place of the nearer pattern, you will find that the point of convergence is far nearer than the apparent place of the pattern.

When you now move your head the pattern moves also. If you have increased the convergence of the eyes, the pattern moves with the eyes, as well to the left and the right as up and down and forwards and backwards. If you diverge, it goes in opposite direction to your head.

LXVII. *Note on Thermal Electrolysis.*

By J. H. Gladstone, *F.R.S.*, *and* Alfred Tribe*.

During the course of our experiments on metallic replacements we noticed that some sheet silver, immersed in fused silver chloride, became quickly studded with crystals of metal. A replacement of a metal by itself seemed so anomalous, that our first idea was that the silver employed contained certain impurities; but we found that the action took place just as well with the purest silver we could obtain, and that it was not restricted to the substances above mentioned. Not only might the iodide of silver be substituted for the chloride with the same result, though not so rapidly effected, but other metals might be employed. Thus, when copper was immersed in fused cuprous chloride, crystals of that metal separated; and similar exchanges took place when zinc was placed in melted zinc chloride, or iron in ferrous chloride in a molten condition.

It was then thought that a different physical condition

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of the rolled metals might give rise to the action; but this was disproved by the following experiments:—

Some crystals of silver prepared by electrolysis were placed in the open end of a piece of glass tubing slightly constricted, and then immersed in silver chloride heated in a crucible by a Bunsen lamp. In about half an hour the crystals were found to have grown in a net-like mass from their original position to a point about half an inch higher in the tube. This experiment was repeated with crystals of silver which had themselves been deposited from the fused chloride by means of metallic silver. A similar result was obtained.

We were then led to the conclusion that the change depended upon the unequal heating of different parts of the immersed metal, or rather of the salt in which it was immersed. It is evident that upon the contact theory of voltaic action, there will be a difference of potential between the metal and the liquid chloride with which it is in contact; and it is in accordance with analogy to suppose that this difference of potential will vary according to the temperature. Now, under the conditions of the experiment, it cannot be supposed that all parts of the fused chloride in contact with the immersed metal were always equally heated; and we have therefore the possibility of a current being established with the consequent electrolysis of the salt.

In order to test this view, some silver chloride was fused in a hard glass tube and a rod of silver placed in the liquid. On heating the underside of the lower end for 10 minutes, we found a considerable crop of silver crystals in the comparatively cool part of the fluid.

In another experiment some silver chloride was fused in a crucible, and one side of the vessel was more strongly heated than the other. Two long rods of silver were connected with a galvanometer and placed, one in the hotter, the other in the colder part of the chloride. The latter was found studded with crystals at the end of 15 minutes, whilst the former was quite clean. On repeating this experiment, it was always found that the galvanometer gave a larger deflection the greater the difference of temperature between the portions of the fused mass penetrated by the silver wires, and that the current was reversed with a reversal of the rods. Copper wires in cuprous chloride gave similar results.

In an experiment with an electrometer we obtained a clear indication of a difference of potential between silver rods in hotter and colder parts of silver chloride fused in a small crucible, the deflection showing a difference of possibly $\frac{1}{50}$ of a

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volt. The reversal of the rods again produced a reversal of the deflection.

In corroboration of the theory above stated, it should be borne in mind that the chlorides of silver, copper, zinc, and iron, when fused, are electrolytes. The liquid chloride of tin is not an electrolyte; and it was found that on immersing tin in this liquid no deposition of crystals was observed when it was so arranged that one part of the liquid was kept at the heat of boiling water and another at the ordinary temperature for two days; nor was there the least action on a galvanometer when arrangements were made for testing by that instrument.

These experiments form a good lecture-table illustration of the conversion of heat into electricity and chemical force. They also seem to have a bearing on the theory of voltaic action, since, from the nature of the substances employed, it is difficult to imagine that chemical action in any way initiates the current.

LXVIII. *Upon the Production of Sound by Radiant Energy.* By Alexander Graham Bell *.

[Plates X. & XI.]

In a paper read before the American Association for the Advancement of Science last August, I described certain experiments made by Mr. Sumner Tainter and myself which had resulted in the construction of a "*Photophone*," or apparatus for the production of sound by light †; and it will be my object today to describe the progress we have made in the investigation of photophonic phenomena since the date of this communication.

In my Boston paper the discovery was announced that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of the various substances which became sonorous in the condition of thin diaphragms; but this failure was explained

* From advance proofs of a paper read before the National Academy of Arts and Sciences, April 21, 1881, communicated by the Author.

† Proceedings of the American Association for the Advancement of Science, August 27th, 1880; see, also, American Journal of Science, vol. xx. p. 305; Journal of the American Electrical Society, vol. iii. p. 3; Journal of the Society of Telegraph Engineers and Electricians, vol. ix. p. 404; *Annales de Chimie et de Physique*, vol. xxi.

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upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was therefore supposed that, if we could lead to the ear air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. The first experiments made to verify this hypothesis pointed towards success. A beam of sunlight was focused into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam, a clear, musical tone was heard, the pitch of which depended upon the frequency of the interruption of the light, and the loudness upon the material composing the tube.

At this stage our experiments were interrupted, as circumstances called me to Europe.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds produced by masses, but would also permit us to test the more general proposition, that *sonorousness, under the influence of intermittent light, is a property common to all matter.*

The substance to be tested was to be placed in the interior of a transparent vessel, made of some material which (like glass) is transparent to light, but practically opaque to sound. Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing-tube.

Some preliminary experiments were made in Paris to test this idea; and the results were so promising that they were communicated to the French Academy on the 11th of October, 1880, in a note read for me by M. Antoine Breguet*. Shortly afterwards I wrote to Mr. Tainter, suggesting that he should carry on the investigation in America, as circumstances prevented me from doing so myself in Europe. As these experiments seem to have formed the common starting-point for a series of independent researches of the most important character, carried on simultaneously, in America by Mr. Tainter, and in Europe by M. Mercadier †, Prof. Tyndall ‡,

* *Comptes Rendus*, vol. xcl. p. 595.

† "Notes on Radiophony," *Comptes Rendus*, Dec. 6 and 13, 1880, and Feb. 21 and 28, 1881; Phil. Mag. Jan. 1881, p. 78. See also *Journal de Physique*, vol. x. p. 53.

‡ "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter," Proc. Royal Society, Jan. 13, 1881, vol. xxxi. p. 307.

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W. C. Röntgen *, and W. H. Preece †, I may be permitted to quote from my letter to Mr. Tainter the passage describing the experiments referred to :—

"Metropolitan Hotel,
Rue Cambon, Paris,
Nov. 2, 1880.

"DEAR MR. TAINTER,— . . . . I have devised a method of producing sounds by the action of an intermittent beam of light from substances that cannot be obtained in the shape of thin diaphragms or in the tubular form ; indeed, the method is specially adapted to testing the generality of the phenomenon we have discovered, as it can be adapted to solids, liquids, and gases.

"Place the substance to be experimented with in a glass test-tube, connnect a rubber tube with the mouth of the test-tube, placing the other end of the pipe to the ear. Then focus the intermittent beam upon the substance in the tube. I have tried a large number of substances in this way with great success, although it is extremely difficult to get a glimpse of the sun here, and when it does shine the intensity of the light is not to be compared with that to be obtained in Washington. I got splendid effects from crystals of bichromate of potash, crystals of sulphate of copper, and from tobacco-smoke. A whole cigar placed in the test-tube produced a very loud sound. I could not hear any thing from plain water ; but when the water was discolored with ink a feeble sound was heard. I would suggest that you might repeat these experiments and extend the results," &c. &c.

Upon my return to Washington in the early part of January ‡, Mr. Tainter communicated to me the results of the experiments he had made in my laboratory during my absence in Europe.

He had commenced by examining the sonorous properties of a vast number of substances enclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard rigid bodies like crystals, or diaphragms such as we had hitherto used.

In order to study the effects under better circumstances, he enclosed his materials in a conical cavity in a piece of brass

* "On the Tones which arise from the intermittent illumination of a gas," see *Annalen der Phys. und Chemie*, Jan. 1881, No. 1, p. 155 ; Phil. Mag. April 1881, p. 308.

† "On the Conversion of Radiant Energy into Sonorous Vibrations,' Proc. Royal Society, March 10, 1881, vol. xxxi. p. 506.

‡ On the 7th of January.

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closed by a flat plate of glass. A brass tube leading into the cavity served for connexion with the hearing-tube. When this conical cavity was stuffed with worsted or other fibrous materials, the sounds produced were much louder than when a test-tube was employed. This form of receiver is shown in figure 1.

Mr. Tainter next collected silks and worsteds of different colours, and speedily found that the darkest shades produced the best effects. Black worsted especially gave an extremely loud sound.

As white cotton-wool had proved itself equal, if not superior, to any other white fibrous material before tried, he was anxious to obtain coloured specimens for comparison. Not having any at hand, however, he tried the effect of darkening some cotton-wool with lampblack. Such a marked reinforcement of the sound resulted that he was induced to try lampblack alone.

About a teaspoonful of lampblack was placed in a test-tube and exposed to an intermittent beam of sunlight. The sound produced was much louder than any heard before.

Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lampblack surface towards the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lampblack surface turned from the sun, the sound was much feebler.

Mr. Tainter repeated these experiments for me immediately upon my return to Washington, so that I might verify his results.

Upon smoking the interior of the conical cavity shown in figure 1, and then exposing it to the intermittent beam, with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube.

The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver, as illustrated in the drawing, figure 1.

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor reinforcements were constantly occurring, which became more

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and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The effects produced by lampblack seemed to me be very extraordinary, especially as I had a distinct recollection of experiments made in the summer of 1880 with smoked diaphragms, in which no such reinforcement was noticed.

Upon examining the records of our past photophonic experiments, we found in vol. vii. p. 57, the following note:—

"Experiment V.—Mica diaphragm covered with lampblack on side exposed to light.

"Result: distinct sound about same as without lampblack.—*A. G. B., July* 18*th*, 1880.

"Verified the above, but think it somewhat louder than when used without lampblack.—*S. T., July* 18*th*, 1880."

Upon repeating this old experiment we arrived at the same result as that noted. Little if any augmentation of sound resulted from smoking the mica. In this experiment the effect was observed by placing the mica diaphragm against the ear, and also by listening through a hearing-tube, one end of which was closed by the diaphragm. The sound was found to be more audible through the free air when the ear was placed as near to the lampblack surface as it could be brought without shading it.

At the time of my communication to the American Association I had been unable to satisfy myself that the substances which had become sonorous under the direct influence of intermittent sunlight were capable of reproducing the sounds of articulate speech under the action of an undulatory beam from our photophonic transmitter. The difficulty in ascertaining this will be understood by considering that the sound emitted by thin diaphragms and tubes were so feeble that it was impracticable to produce audible effects from substances in these conditions at any considerable distance away from the transmitter; but it was equally impossible to judge of the effects produced by our articulate transmitter at a short distance away, because the speaker's voice was directly audible through the air. The extremely loud sounds produced from lampblack have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed.

The drawing, fig. 2, illustrates the mode in which the experiment was conducted. The diaphragm of the transmitter (A) was only 5 centimetres in diameter; the diameter of the

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receiver (B) was also 5 centimetres; and the distance between the two was 40 metres, or 800 times the diameter of the transmitting diaphragm. We were unable to experiment at greater distances without a heliostat, on account of the difficulty of keeping the light steadily directed on the receiver. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lampblack receiver.

In fig. 3 is shown a mode of interrupting a beam of sunlight for producing distant effects without the use of lenses. Two similarly-perforated disks are employed, one of which is set in rapid rotation, while the other remains stationary. This form of interrupter is also admirably adapted for work with artificial light. The receiver illustrated in the drawing consists of a parabolic reflector, in the focus of which is placed a glass vessel (A) containing lampblack or other sensitive substance, and connected with a hearing-tube. The beam of light is interrupted by its passage through the two slotted disks shown at B; and in operating the instrument, musical signals like the dots and dashes of the Morse alphabet are produced from the sensitive receiver (A) by slight motions of the mirror (C) about its axis (D).

In place of the parabolic reflector shown in the figure, a conical reflector like that recommended by Prof. Silvanus Thompson * can be used, in which case a cylindrical glass vessel would be preferable to the flask (A) shown in the figure.

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the colour are two conditions that markedly influence the intensity of the sonorous effects. *The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colours.*

The materials from which the best effects have been produced are cotton-wool, worsted, fibrous materials generally, cork, sponge, platinum and other metals in a spongy condition, and lampblack.

The loud sounds produced from such substances may perhaps be explained in the following manner. Let us consider, for example, the case of lampblack—a substance which becomes heated by exposure to rays of all refrangibilities. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass, the particles of lampblack are heated,

* Phil. Mag. April 1881, vol. xi. p. 286.

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and consequently expand, causing a contraction of the air-spaces or pores among them.

Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge.

The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lampblack. When the light is cut off, the converse process takes place. The lampblack particles cool and contract, thus enlarging the air-spaces among them, and the enclosed air also becomes cool. Under these circumstances a partial vacuum should be formed among the particles, and the outside air would then be absorbed, as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon the lampblack, and a wave of rarefaction is originated when the light is cut off. *We can thus understand how it is that a substance like lampblack produces intense sonorous vibrations in the surrounding air, while at the same time it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.*

This curious fact was independently observed in England by Mr. Preece; and it led him to question whether, in our experiments with thin diaphragms, the sound heard was due to the vibration of the disk, or (as Prof. Hughes had suggested) to the expansion and contraction of the air in contact with the disk confined in the cavity behind the diaphragm. In his paper read before the Royal Society on the 10th of March, Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to the vibrations of the confined air, and that the *disks do not vibrate at all.*

I shall briefly state my reasons for disagreeing with him in this conclusion.

1. When an intermittent beam of sunlight is focused upon a sheet of hard rubber or other material, a musical tone can be heard, not only by placing the ear immediately behind the part receiving the beam, but by placing it against any portion of the sheet, even though this may be a foot or more from the place acted upon by the light.

2. When the beam is thrown upon the diaphragm of a "Blake transmitter," a loud musical tone is produced by a telephone connected in the same galvanic circuit with the carbon button (A), fig. 4. Good effects are also produced when the carbon button (A) forms, with the battery (B), a portion of the primary circuit of an induction-coil, the telephone (C) being placed in the secondary circuit.

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In these cases the wooden box and mouth-piece of the transmitter should be removed, so that no air-cavities may be left on either side of the diaphragm.

It is evident, therefore, that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, independently of any expansion and contraction of the air confined in the cavity behind the diaphragm.

Lord Rayleigh has shown mathematically that a to-and-fro vibration, of sufficient amplitude to produce an audible sound, would result from a periodical communication and abstraction of heat; and he says:—"We may conclude, I think, that there is at presemt no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating" ('Nature,' xxiii. p. 274). Mr. Preece, however, seeks to prove that the sonorous effects cannot be explained upon this supposition; but his experimental proof is inadequate to support his conclusion. Mr. Preece expected that if Lord Rayleigh's explanation was correct, the expansion and contraction of a thin strip under the influence of an intermittent beam could be caused to open and close a galvanic circuit so as to produce a musical tone from a telephone in the circuit. But this was an inadequate way to test the point at issue; for Lord Rayleigh has shown (Proc. of Roy. Soc. 1877) that an audible sound can be produced by a vibration whose amplitude is *less than a ten-millionth of a centimetre*; and certainly such a vibration as that would not have sufficed to operate a "make-and-break contact" like that used by Mr. Preece. The negative results obtained by him cannot, therefore, be considered conclusive.

The following experiments (devised by Mr. Tainter) have given results decidedly more favourable to the theory of Lord Rayleigh than to that of Mr. Preece:—

1. A strip (A) similar to that used in Mr. Preece's experiment was attached firmly to the centre of an iron diaphragm (B), as shown in figure 5, and was then pulled taut at right angles to the plane of the diaphragm. When the intermittent beam was focused upon the strip (A), a clear musical tone could be heard by applying the ear to the hearing-tube (C).

This seemed to indicate a rapid expansion and contraction of the substance under trial.

But a vibration of the diaphragm (B) would also have resulted if the thin strip (A) had acquired a to-and-fro motion, due either to the direct impact of the beam or to the sudden expansion of the air in contact with the strip.

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2. To test whether this had been the case, an additional strip (D) was attached by its central point only to the strip under trial, and was then submitted to the action of the beam, as shown in fig. 6.

It was presumed that, if the vibration of the diaphragm (B) had been due to a *pushing force* acting on the strip (A), the addition of the strip (D) would not interfere with the effect; but if, on the other hand, it had been due to the longitudinal expansion and contraction of the strip (A), the sound would cease, or at least be reduced. The beam of light falling upon strip (D) was now interrupted as before by the rapid rotation of a perforated disk, which was allowed to come gradually to rest.

No sound was heard excepting at a certain speed of rotation, when a feeble musical tone became audible.

This result is confirmatory of the first.

The audibility of the effect at a particular rate of interruption suggests the explanation that the strip D had a normal rate of vibration of its own. When the frequency of the interruption of the light corresponded to this, the strip was probably thrown into vibration after the manner of a tuning-fork, in which case a to-and-fro vibration would be propagated down its stem or central support to the strip (A).

This indirectly proves the value of the experiment.

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here; and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment*.

Experiments with Liquids.

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absorptive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light; but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment. In the experiments made in my laboratory, a very long test-tube was filled with the liquid under examination, and a flexible-rubber tube was

* Carbon and thin microscope glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy (*Comptes Rendus*, vol. xcl. p. 595). All these substances have since yielded sounds under more careful conditions of experiment.

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slipped over the mouth far enough down to prevent the possibility of any light reaching the vapour above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focused upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

Results.

Clear water.....	No sound audible.
Water discoloured by ink ...	Feeble sound.
Mercury	No sound heard.
Sulphuric ether*.....	Feeble but distinct sound.
Ammonia	" " " "
Ammonio-sulphate of copper	" " " "
Writing-ink	" " " "
Indigo in sulphuric acid ...	" " " "
Chloride of copper *	" " " "

The liquids distinguished by an asterisk gave the best sounds.

Acoustic vibrations are always much enfeebled in passing from liquids to gases; and it is probable that a form of experiment may be devised which will yield better results, by communicating the vibrations of the liquid to the ear through the medium of a solid rod.

Experiments with Gaseous Matter.

On the 29th of November, 1880, I had the pleasure of showing to Prof. Tyndall, in the laboratory of the Royal Institution, the experiments described in the letter to Mr. Tainter, from which I have quoted above; and Prof. Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapour of sulphuric ether (a good absorbent of heat), and another with the vapour of bisulphide of carbon (a poor absorbent); and he predicted that, if any sound was heard, it would be louder in the former case than in the latter.

The experiment was immediately made; and the result verified the prediction.

Since the publication of the memoirs of Röntgen † and Tyndall ‡ we have repeated these experiments, and have ex-

† *Ann. der Phys. und Chem.* 1881, No. 1, p. 155.
‡ Proc. Roy. Soc. vol. xxxi. p. 307.

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tended the inquiry to a number of other gaseous bodies, obtaining in every case similar results to those noted in the memoirs referred to.

The vapours of the following substances were found to be highly sonorous in the intermittent beam — water vapour, coal-gas, sulphuric ether, alcohol, ammonia, amylene, ethyl bromide, diethylamine, mercury, iodine, and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen.

I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition (solid, liquid, and gaseous); and the probability is therefore very greatly increased that sonorousness under such circumstances will be found to be a universal property of matter.

Upon Substitutes for Selenium in Electrical Receivers.

At the time of my communication to the American Association, the loudest effects obtained were produced by the use of selenium, arranged in a cell of suitable construction, and placed in a galvanic circuit with a telephone. Upon allowing an intermittent beam of sunlight to fall upon the selenium, a musical tone of great intensity was produced from the telephone connected with it.

But the selenium was very inconstant in its action. It was rarely, if ever, found to be the case that two pieces of selenium (even of the same stick) yielded the same results under identical circumstances of annealing &c. While in Europe last autumn, Dr. Chichester Bell, of University College, London, suggested to me that this inconstancy of result might be due to chemical impurities in the selenium used. Dr. Bell has since visited my laboratory in Washington, and has made a chemical examination of the various samples of selenium I had collected from different parts of the world. As I understand it to be his intention to publish the results of this analysis very soon, I shall make no further mention of his investigation than to state that he has found sulphur, iron, lead, and arsenic in the so-called "selenium," with traces of organic matter, that a quantitative examination has revealed the fact that sulphur constitutes nearly one per cent. of the whole mass, and that when these impurities are eliminated, the selenium appears to be more constant in its action and more sensitive to light.

Prof. W. G. Adams * has shown that tellurium, like sele-

* Proc. Roy. Soc. vol. xxiv. p. 163.

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nium, has its electrical resistance affected by light; and we have attempted to utilize this substance in place of selenium. The arrangement of cell shown in fig. 7 was constructed for this purpose in the early part of 1880; but we failed at that time to obtain any indications of sensitiveness with a reflecting galvanometer. We have since found, however, that when this tellurium spiral is connected in circuit with a galvanic battery and telephone, and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone. The audible effect is much increased by placing the tellurium-cell with the battery in the primary circuit of an induction-coil, and placing the telephone in the secondary circuit.

The enormously high resistance of selenium and the extremely low resistance of tellurium suggested the thought that an alloy of these two substances might possess intermediate electrical properties. We have accordingly mixed together selenium and tellurium in different proportions; and while we do not feel warranted at the present time in making definite statements concerning the results, I may say that such alloys have proved to be sensitive to the action of light.

It occurred to Mr. Tainter, before my return to Washington last January, that the very great molecular disturbance produced in lampblack by the action of intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lampblack could be employed in place of selenium in an electrical receiver. This has turned out to be the case; and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

The form of lampblack-cell we have found most effective is shown in fig. 8. Silver is deposited upon a plate of glass; and a zigzag line is then scratched through the film, as shown, dividing the silver surface into two portions insulated from one another, having the form of two combs with interlocking teeth.

Each comb is attached to a screw-cup, so that the cell can be placed in an electrical circuit when required. The surface is then smoked until a good film of lampblack is obtained, filling the interstices between the teeth of the silver combs. When the lampblack-cell is connected with a telephone and galvanic battery, and exposed to the influence of an intermittent beam of sunlight, a loud musical tone is produced by the telephone. This result seems to be due rather to the physical condition than to the nature of the conducting material employed, as metals in a spongy condition produce

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similar effects. For instance, when an electrical current is passed through spongy platinum while it is exposed to intermittent sunlight, a distinct musical sound is produced by a telephone in the same circuit. In all such cases the effect is increased by the use of an induction-coil; and the sensitive cells can be employed for the reproduction of articulate speech as well as for the production of musical sounds.

We have also found that loud sounds are produced from lampblack by passing through it an intermittent electrical current, and that it can be used as a telephonic receiver for the reproduction of articular speech by electrical means.

A convenient mode of arranging a lampblack cell for experimental purposes is shown in fig. 9. When an intermittent current is passed through the lampblack (A), or when an intermittent beam of sunlight falls upon it through the glass plate B, a loud musical tone can be heard by applying the ear to the hearing-tube C. When the light and the electrical current act simultaneously, two musical tones are perceived, which produce beats when nearly of the same pitch. By proper arrangements a complete interference of sound can undoubtedly be produced.

Upon the Measurement of the Sonorous Effects produced by Different Substances.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment; and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects; but as our researches are not yet complete, I shall confine myself to a simple description of some of the forms of apparatus we have devised.

When a beam of light is brought to a focus by means of a lens, the beam diverging from the focal point becomes weaker as the distance increases, in a calculable degree. Hence, if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity, we can calculate their relative sonorous powers.

Preliminary experiments were made by Mr. Tainter during my absence in Europe, to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between different substances in this respect.

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Distance from Focal Point of Lens at which Sounds become Inaudible with Different Substances.

	millim.
Zinc diaphragm (polished)	1·51
Hard-rubber diaphragm	1·90
Tin-foil „	2·00
Telephone „ (japanned iron)	2·15
Zinc „ (unpolished)	2·15
White silk (in receiver shown in fig. 1)	3·10
White worsted „ „ „	4·01
Yellow worsted „ „ „	4·06
Yellow silk „ „ „	4·13
White cotton-wool „ „ „	4·38
Green silk „ „ „	4·52
Blue worsted „ „ „	4·69
Purple silk „ „ „	4·82
Brown silk „ „ „	5·02
Black silk „ „ „	5·21
Red silk „ „ „	5·24
Black worsted „ „ „	6·50
Lampblack. In receiver the limit of audibility could not be determined on account of want of space. Sound perfectly audible at a distance of	10·00

Mr. Tainter was convinced from these experiments that this field of research promised valuable results; and he at once devised an apparatus for studying the effects, which he described to me upon my return from Europe. The apparatus has since been constructed; and I take great pleasure in showing it to you today.

(1) A beam of light is received by two similar lenses (A, B, fig. 10*), which bring the light to a focus on either side of the interrupting-disk (C). The two substances whose sonorous powers are to be compared are placed in the receiving vessels (D, E) (so arranged as to expose equal surfaces to the action of the beam) which communicate by flexible tubes (F, G), of equal length, with the common hearing-tube (H). The receivers (D, E) are placed upon slides, which can be moved along the graduated supports (I, K). The beams of light passing through the interrupting-disk (C) are alternately cut off by the swinging of a pendulum (L). Thus a musical sound is produced alternately from the substance in D and from that in E. One of the receivers is kept at a constant point upon its scale; and the other receiver is moved towards or from the focus of its beam until the ear decides that the sounds pro-

[* The letters in this figure were omitted by the author, and the omission unfortunately was not detected until after the Plate was printed; the reader can easily supply them.—Ed. *Phil. Mag.*]

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duced from D and E are of equal intensity. The relative positions of the receivers are then noted.

(2) Another method of investigation is based upon the production of an interference of sound; and the apparatus employed is shown in fig. 11. The interrupter consists of a tuning-fork (A), which is kept in continuous vibration by means of an electromagnet (B).

A powerful beam of light is brought to a focus between the prongs of the tuning-fork (A); and the passage of the beam is more or less obstructed by the vibration of the opaque screens (C, D) carried by the prongs of the fork.

As the tuning-fork (A) produces a sound by its own vibration, it is placed at a sufficient distance away to be inaudible through the air; and a system of lenses is employed for the purpose of bringing the undulating beam of light to the receiving lens (E) with as little loss as possible. The two receivers (F, G) are attached to slides (H, I), which move upon opposite sides of the axis of the beam; and the receivers are connected by flexible tubes of unequal length (K, L) communicating with the common hearing-tube (M).

The length of the tube (K) is such that the sonorous vibrations from the receivers (F, G) reach the common hearing-tube (M) in opposite phases. Under these circumstances silence is produced when the vibrations in the receivers (F, G) are of equal intensity. When the intensities are unequal, a residual effect is perceived. In operating the instrument the position of the receiver (G) remains constant, and the receiver (F) is moved to or from the focus of the beam until complete silence is produced. The relative positions of the two receivers are then noted.

(3) Another mode is as follows:—The loudness of a musical tone produced by the action of light is compared with the loudness of a tone of similar pitch produced by electrical means. A rheostat introduced into the circuit enables us to measure the amount of resistance required to render the electrical sound equal in intensity to the other.

(4) If the tuning-fork (A) in fig. 11 is thrown into vibration by an undulatory instead of an intermittent current passed through the electromagnet (B), it is probable that a musical tone, electrically produced in the receiver (F) by the action of the same current, would be found capable of extinguishing the effect produced in the receiver (G) by the action of the undulatory beam of light, in which case it should be possible to establish an acoustic balance between the effects produced by light and electricity, by introducing sufficient resistance into the electric circuit.

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Upon the Nature of the Rays that produce Sonorous Effects in Different Substances.

In my paper read before the American Association last August, and in the present paper, I have used the word "light" in its usual rather than its scientific sense; and I have not hitherto attempted to discriminate the effects produced by the different constituents of ordinary light—the thermal, luminous, and actinic rays. I find, however, that the adoption of the word "photophone" by Mr. Tainter and myself has led to the assumption that we believed the audible effects discovered by us to be due entirely to the action of luminous rays. The meaning we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage, quoted from my Boston paper :—

"Although effects are produced as above shown by forms of radiant energy which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone,' *because an ordinary beam of light contains the rays which are operative.*"

To avoid in future any misunderstandings upon this point, we have decided to adopt the term "*radiophone*," proposed by M. Mercadier, as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone*, *photophone*, and *actinophone* to apparatus for the production of sound by thermal, luminous or actinic rays respectively.

M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum (*Comptes Rendus*, Dec. 6th, 1880).

We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier.

(1) A beam of sunlight was reflected from a heliostat (A, fig. 12) through an achromatic lens (B), so as to form an image of the sun upon the slit (C).

The beam then passed through another achromatic lens (D) and through a bisulphide-of-carbon prism (E), forming a spectrum of great intensity, which, when focused upon a screen, was found to be sufficiently pure to show the principal absorption-lines of the solar spectrum.

The disk interrupter (F) was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the

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receiver (G), which was so arranged that the lampblack surface exposed was limited by a slit, as shown.

Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver (G) gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to decrease, and then stopped so suddenly that a very slight motion of the receiver (G) made all the difference between almost maximum sound and complete silence.

(2) The lampblacked wire gauze was then removed, and the interior of the receiver (G) was filled with red worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green, at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

(3) Upon substituting green silk for red worsted, the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

(4) Some hard-rubber shavings were now placed in the receiver (G). The limits of audibility appeared to be, on the one hand, the junction of the green and blue, and, on the other, the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red; and to his ear the maximum was about the junction of the red and orange.

(5) A test-tube containing the vapour of sulphuric ether was then substituted for the receiver (G). Commencing at the violet end, the test-tube was gradually moved down the spectrum, and out into the ultra-red, without audible effect; but when a certain point far out in the ultra-red was reached a distinct musical tone suddenly made its appearance, which disappeared as suddenly on moving the test-tube a very little further on.

(6) Upon exploring the spectrum with a test-tube containing the vapour of iodine, the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

(7) A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were ob-

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tained in all parts of the visible spectrum, but no sounds were observed in the ultra-red.

The maximum effect seemed to me to be in the blue. The sounds were well-marked in all parts of the violet; and I even fancied that the audible effect extended a little way into the ultra-violet; but of this I cannot be certain. Upon examining the absorption-spectrum of peroxide of nitrogen, it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption-lines made their appearance.

(8) The spectrum was now explored by a selenium-cell; and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red. The audible effect extended a little way into the ultra-red, on the one hand, and up as high as the middle of the violet, on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that *the nature of the rays that produce sonorous effects in different substances depends upon the nature of the substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.*

The Spectrophone.

Our experiments upon the range of audibility of differen substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis, which was described and exhibited to the Philosophical Society of Washington last Saturday *. The eye-piece of a spectroscope is removed; and sensitive substances are placed in the focal point of the instrument, behind an opaque diaphragm containing a slit. These substances are put in communication with the ear by means of a hearing-tube; and thus the instrument is converted into a veritable "spectrophone," like that shown in fig. 13.

Suppose we smoke the interior of our spectrophonic receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum (visible and invisible), except the ultra-violet. Now pass a rapidly-interrupted beam of light through some substances whose absorption-spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption-

* Proc. of Phil. Soc. of Washington, April 16, 1881.

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bands. Of course, the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum; but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum, lampblack alone may be used in the spectrophonic receiver. Indeed the sounds produced by this substance in the ultra-red are so well-marked as to constitute our instrument a most reliable and convenient substitute for the thermopile. A few experiments that have been made may be interesting.

(1) The interrupted beam was filtered through a saturated solution of alum.

Result. The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

(2) A thin sheet of hard rubber was interposed in the path of the beam.

Result. Well-marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time. (See table of results in fig. 14.)

(3) A solution of ammonia-sulphate of copper was tried.

Result. When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear, however, the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination; but I do not wish you to understand that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man; and I feel that *our* first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognize the fact that the spectrophone must ever remain a mere adjunct to the spectroscope; but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption-spectra in the ultra-red.

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LXIX. *On Action at a Distance, and the Conservation of Energy.* *By* Oliver J. Lodge, *D.Sc.**

MR. BROWNE'S letter of last month scarcely calls for a reply; but it may be well, without opening up any fresh points of controversy, to indicate the errors in his statements quite briefly; and I trust that he will acquit me of any desire to import personal animus into the controversy. If I criticise his remarks in an unceremonious manner, it is partly for the sake of conciseness, and also because, notwithstanding all personal respect, I feel so strongly that his reasoning is utterly wrong, that I should be conveying a wrong impression if I used the language of compliment and ceremony in attacking it.

Mr. Browne first quotes my argument, by which as he says I do solemnly imagine that I have "disposed for ever of the idea of action at a distance," and then gives four fresh confutations of it, which he labels *a*, *b*, *c*, and *d*. Let us refer to them on page 380, and take them in order.

(*a*) In the fifth and sixth lines of this paragraph it is necessary to replace the word "of" by the words "generated by this force in," to make it agree with the statement (in the first two lines) of how force is measured. The concluding three lines from "Hence" to the end may then be omitted.

(*b*) Energy is undoubtedly a positive quantity; but *gain* of energy may be either positive or negative. To be consistent Mr. Browne ought to deny that distance travelled could ever be reckoned negative, because the distance between two places is an essentially positive quantity. But it looks childish to have to point this out.

(*c*) Two equal *particles* of finite mass impinging on one another with equal opposite velocities of course exert an infinite stress and stop instantaneously; so there is no difficulty with *them*.

If Mr. Browne had chosen to consider the impact of two finite bodies he would have been more troublesome, because their action is complex: the particles which first touch stop dead; but the others are gradually stopped by actions transmitted from particle to particle in a manner not by any means yet precisely understood.

But direct impact is an unfortunate example to fly to in order to support action at a distance.

(*d*) Here we come to the gist of the matter, and to the "real and intrinsic vice" of my argument. He is getting near the scent now when he implies that my statement of the

* Communicated by the Author.

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conservation of energy is not identical with that given in the text-books; but he goes off it again. I assure him that I really do not assume that conservation is true of potential energy only, and forget to take account of kinetic: I should be rather mad if I did.

A case is then put "in mathematical form;" and by the ingenious, but rather cheap, device of writing $v\,dv$ in the shape

$$\frac{dx}{dt}\frac{d}{dt}\left(\frac{dx}{dt}\right)\delta t$$

and then mixing together the energy-equations for *two distinct particles* into one equation, an expression intended to be alarming is manufactured. But, unfortunately, Mr. Browne starts by saying that I "suppose two particles moving with different velocities v and v', and acting on each other with equal and opposite moving forces F." Now, with all due deference, this is exactly what I do *not* suppose. For as long as two particles are moving with different velocities they cannot be in contact; and my case is, that two bodies not in contact are *ipso facto* incompetent to act directly on each other; it is only when they touch and during the period of contact (that is, while they are both moving at the same rate in the same direction) that any *immediate* action can take place between them.

But now that we have come so near to the real peculiarity in my argument, perhaps we had better have it out, not for the sake of prolonging the present controversy, nor, indeed, with any reference to it, but because I do undoubtedly attach importance to the matter, and distinctly believe that it proves direct action at a distance to be impossible. Moreover, I am of the opinion that this is not a metaphysical or trivial question, but an essentially physical one, and one that is by no means unimportant just now, when opposition theories of electricity—the medium-theory of Maxwell and Faraday, and the action-at-a-distance theories of Weber, Gauss, Neumann, and others—are in the field against each other.

Theories of physical phenomena worked out on the hypothesis of direct forces across intervening space are, at certain stages of our knowledge, an immense help and of great importance—notably the gravitation theory of Newton and the electrical theories of Coulomb and Ampère; but if it can once be distinctly proved and clearly recognized that they *can* only be provisional, and must necessarily be replaced by medium theories as the science progresses, a useful step will have been made; and one result will be that there can be no further question as to which of the rival electrical theories

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(Maxwell's or those of the Germans) enters most closely into the actual nature of the phenomena, and is the nearest approach towards the ultimate solution of the problem.

Now, my first contention is this, that any one who admits my statement of the conservation of energy*, and also the ordinary statement of the equality of action and reaction, is bound to admit my conclusion.

And my second contention is that, whether my statement is identical with that of the "text-books" or not, it is a reasonable and true statement, that it is just as axiomatic as any other, and that it ought to be accepted.

This second contention, however, is distinctly open to discussion.

Now, I have pointed out in section 84 of my little book on Mechanics, and on page 280 of the Phil. Mag. for 1879, that the two fundamental forms of energy correspond to the two factors in the product *work*. A body exerting force possesses energy, and a body moving through space possesses energy, the first form being called potential, or dynamic, or static, the second being called kinetic; but a body is not doing work unless it is *both* exerting force *and* moving through space; and in this case it is losing energy and transferring it to the body upon which the work is being done, the energy at the same time being always transformed from kinetic to potential, or *vice versâ*.

Consider, for instance, the earth and a stone (or any other "attracting" bodies) supposed at rest in space, and separated from each other against whatever it be that is called their mutual attraction. A common mode of stating the condition of affairs is to state that the stone possesses potential or possible energy, which will become actual when it is allowed to fall, the sum of the possible and actual energies remaining constant during the whole time of fall, until, in fact, they are knocked out of the body by the blow. This, however, is the *very* commonest mode of stating the matter; and I suppose nearly every one will agree that the phrases actual and possible energy are little better than nonsense, or, at any rate, that any law of *conservation* of energy founded upon such notions must be utterly meaningless.

* Phil. Mag. Jan. 1881, p. 36, and Oct. 1879, p. 278. It may be repeated for convenience—The energy of a body is increased whenever work is done *upon* it, and diminished whenever work is done *by* it, by an amount in each case numerically equal to the work done; but whenever two bodies act on each other the work done by the one is identical with that done upon the other; in other words, energy is never generated or destroyed by such actions, but is simply transferred from one body to the other.

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Another mode of expressing the matter is to say that the stone possesses potential energy by virtue of its position, just as a bullet possesses energy of position when placed inside a loaded gun, or as an arrow possesses it when strung on a bent bow. But one has carefully to shut one's eyes and think vaguely if one is to maintain this ground with any degree of comfort; for it will be soon perceived that it is not the bullet nor the arrow which really possesses the energy before firing, but the powder and the bow, and that it is difficult to see how a passive stone at rest in its ordinary condition, notwithstanding that it is in an elevated position, can properly be said to possess any energy at all.

The difficulty of supposing that the potential energy belongs to the stone any more than to the earth having been felt, the fact was denied; and it is now orthodox to say that the energy belongs neither to the stone nor to the earth (neither to the bullet nor to the gun), but *to the system in virtue of its configuration.*

Now this is undoubtedly true, and as a mere abstract and mathematical way of stating the facts it is sufficient; but on examining it more closely one perceives that it is only a mystical way of saying that the energy really belongs to the medium which is driving the two bodies together (or apart)—the strained æther (as I believe), or Le Sage's corpuscles, or Mr. Tolver Preston's gravity-gas, or whatever it may be.

The energy must be possessed by something, unless we go back and say it is only *possible* energy (which is the most barefaced denial of its conservation); and, according to my view, potential energy can only be possessed by a body exerting force; so what is there, then, that can possess the energy but the medium which surrounds the two bodies and which is pressing them together?

This is what is exerting the force; and when the stone is allowed to fall, this is what does the work and transfers its energy both to the stone and earth, though practically all to the stone; so that, just before the blow takes place, the stone is found to possess the whole of the energy in the kinetic form, work having been done upon it during the whole time of fall.

This statement is not identical with that generally accepted; but I maintain strongly that it is the only one which is clear and accurate and devoid of vagueness.

The equation $mgh = \frac{1}{2}mv^2$ is commonly interpreted to mean that the energy of the stone merely changes its form during the fall, no work being done unless the motion is resisted. But I deny that energy can *ever* change its form without

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being at the same time transferred from one body to another by an act of work.

The fact is, that the conservation of energy has no real physical meaning if potential energy is to be regarded *solely* as a mathematically convenient abstraction, or "force-function." It is useful enough in that capacity ; but when one comes to think of things physically, one perceives that it must be a good deal more.

Dr. Schuster first remarked to me that my statement of the conservation of energy, though possibly correct, was not identical with that currently accepted and expressed in familiar equations, but was a slight (very slight) extension of it. I think this is probably so ; but I believe it to be fully as axiomatic as the other ; indeed I believe it to express the real physical facts which give to those equations the whole of their validity.

The essential points on which I wish to lay stress are these :—

1st. That it is impossible to have a force without a body which is exerting that force, and also without another body on which the force is exerted, and which is exerting an equal counter force.

2nd. That it is not the *force* which does work, but the *body* which is exerting the force that does it.

3rd. That the thing which does the work must possess the energy, and, hence, that energy, if existent at all, must be possessed by a body.

4th. That work is always done *by* one body *upon* another, the second body gaining the energy which the first loses ; whence, since they exert on each other equal opposite forces, they must both move together over the same distance, *i. e.* they must touch during the action.

5th. That this latter argument may be worked with greater ease conversely, and instead of proving that universal contact-action holds wherever energy is conserved, one may prove the conservation of energy by assuming universal contact-action.

[This is what I did in the paper in Oct. 1879. The argument is simpler in this form, merely because one thus arrives at the conservation of energy in its (as I believe) true and complete form, while the data assumed are simple and definite ; whereas the converse argument, though equally conclusive in itself, may be eluded by denying the universal applicability of the conservation statement.]

6th. That energy can only be transferred from one body to another by work being done by the first body upon the second.

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7th. That whenever energy is transferred from one body to another it is also always transformed from the potential to the kinetic form, or *vice versâ*.

8th. That energy never changes its form without at the same time being transferred from one body to another by an act of work.

9th (and most important). That the term "body" includes every material thing, whether visible or ponderable or otherwise, and that a piece of matter is to be regarded as a different "*body*" from the ultimate particles of which it is composed; so that when a bullet strikes a target one may say that part of the energy of the bullet is transferred to its particles; and when a spring uncoils suddenly, that the energy of the strained particles is transferred to the mass of the spring—just as easily as one may say that, when a pendulum swings, energy is transferred from one "*body*," the gravitation medium, to another, the bob of the pendulum, or *vice versâ*, at every quarter swing.

LXX. *On some new Aparatus for use in Gas-analysis.* By J. Alfred Wanklyn *and* W. J. Cooper*.

THE analysis of gases by means of measurements over mercury has been brought to a state of great perfection, and is admirably adapted for many purposes. Still there are instances in which this method of procedure is inapplicable.

Such instances are found in those cases where the constituent is very small in amount, or where the gas attacks mercury. There is a very familiar example in the carbonic acid of the atmosphere. Ten thousand volumes of air contain about four volumes of carbonic acid. If the chemist makes a measurement of air confined over mercury, and then absorbs the carbonic acid by potash, and then measures the air freed from carbonic acid, he obtains data from which he might propose to calculate the amount of carbonic acid in the air. But, as has been pointed out on various occasions by different chemists, these data lose their value because the experimental error is almost as large as the difference between the two gas-measurements. Determinations of the carbonic acid in atmospheric air carried out by this method are illusory; and long ago (so long ago as the beginning of the century) another method was resorted to.

The other method consists in exposing a known volume of air to the action of lime-water or baryta-water, and in noting

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the amount of change suffered by the lime- or baryta-water. Sometimes the amount of the insoluble carbonate is weighed, and sometimes the diminution of alkalinity is noted; and the characteristic of this method is that the point to be observed is the change in the reagent, and not in the volume of the gas.

In carrying out these measurements of the carbonic acid in the air the manipulation is of the simplest description. The air is contained in a large stoppered flask or bottle, the capacity of which has been measured. The stopper is taken out, the baryta-water is poured in, and the stopper is replaced. Then the bottle is shaken up for a sufficient length of time; and ultimately the operator takes the stopper out, and deals with the carbonate of baryta or the baryta-water in the well-known manner.

Now let us suppose that the chemist had to measure the carbonic acid in a gas lighter than air. He has his bottle filled with, for example, coal-gas.

It will be apparent that when he tries to pour the baryta-water into the gas he will meet with difficulties. If the mouth of the bottle be held upwards, the gas will escape when the stopper is removed; and, if the mouth be directed downwards, the operator will not be able to pour the baryta-water into the bottle.

Our new apparatus is designed to enable the operator to overcome this difficulty. We make the stopper hollow, and place the reagent in the stopper. Instead of a solid stopper we use a flask of thick glass, the neck of which is ground so as to fit into the neck of the gas-bottle.

The following examples will serve to illustrate our method of operating:—

We had occasion to measure the carbonic acid, sulphuretted hydrogen, and ammonia in crude coal-gas—that is to say, in coal-gas from which the tar and water distilling with the tar had been removed, but which had not been further purified.

The gas in question was collected by displacement in a bottle holding 2750 cubic centim.; and about 50 cubic centim. of strong baryta-water was placed in the little flask which was employed instead of a stopper to close the bottle containing the gas. The apparatus was then shaken, and the resulting carbonate of baryta collected on a filter, washed, ignited, and weighed. From the amount of carbonate the amount of carbonic acid was calculated. In another experiment the sulphuretted hydrogen was measured by using a solution of acetate of lead, the sulphuret of lead being subsequently converted into sulphate of lead, and weighed. In a

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third experiment the ammonia was ascertained by the use of dilute standard acid.

The results are as follows :—In 1000 vols. of crude coal-gas,

	vols.
Carbonic acid.....	15·0
Sulphuretted hydrogen.....	12·1
Ammonia	3·6

The apparatus possesses many practical advantages, which reveal themselves when the operator begins to use it. Thus, the reagent may be drained into the flask, removed from the gas, and a fresh quantity of reagent employed. Successive treatment is even practicable to a certain extent.

LXXI. *Notices respecting New Books.*

Miscellaneous Scientific Papers by W. J. Macquorn Rankine, *C.E., LL.D., F.R.S., late Regius Professor of Civil Engineering and Mechanics in the University of Glasgow. From the Transactions and Proceedings of the Royal and other Scientific and Philosophical Societies, and the Scientific Journals. With a Memoir of the Author by* P. G. Tait, *M.A. Edited by* W. J. Millar, *C.E. With Portrait, Plates, and Diagrams.* London: Charles Griffin and Co., 1881. (Pp. xxxvi and 567.)

AFTER a considerable lapse of time (Rankine died December 24th, 1872) these papers are now issued in a collected form. It is hard to account for so long a delay, seeing that the papers have all been published before, and we cannot suppose any hindrance was interposed by the governing bodies of the Scientific Societies and Journals to whom the papers were originally intrusted by the author. It is sufficient now for us to indicate how many and, to a certain extent, which of the 150 and more papers (*cf.* Royal Society Catalogue of Scientific Papers) are here submitted as worthy of fitly representing Rankine's contributions to science. Their number is thirty-seven, classed under three heads. The first group embraces those papers which relate to Temperature, Elasticity, and Expansion of Vapours, Liquids, and Solids. Of the nine papers, three first saw the light in our pages, viz.:—iii. "On the Centrifugal Theory of Elasticity, as applied to Gases and Vapours" (Dec. 1851); vii. "On the Vibrations of Plane-polarized Light" (June 1851); viii. "General View of an Oscillatory Theory of Light" (Dec. 1853). The longest paper (read before the British Association, August 1850, and published in the Cambridge and Dublin Mathematical Journal, May 1851) is on the "Laws of the Elasticity of Solid Bodies." The second group contains papers relating to Energy and its Transformations, Thermodynamics, Mechanical Action of Heat in the Steam-engine, &c.; they are nineteen in number. In this group also are three Phil. Mag. papers:—x. the remarkable paper

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"On the Reconcentration of the Mechanical Energy of the Universe" (Nov. 1852); xxi. "On Formulæ for the Maximum Pressure and Latent Heat of Vapours;" xxiii. "On the Second Law of Thermodynamics." Here we find grouped a number of the most valuable contributions, as:—xii. "Outlines of the Science of Energetics;" xxiii. "On the Phrase 'Potential Energy,' and on the Definitions of Physical Quantities;" xiv. "On the Mechanical Action of Heat, especially in Gases and Vapours;" xix. "On the Mechanical Action of Heat;" xx. "On the Geometrical Representation of the Expansive Action of Heat, and the Theory of Thermodynamic Engines." The concluding nine papers are on Wave-forms, Propulsion of Vessels, Stability of Structures, &c. In this cluster our Magazine claims two notelets—xxv. "On the application of Barycentric Perspective to the Transformation of Structures" (Nov. 1853), xxvi. "Principle of the Equilibrium of Polyhedral Frames" (Feb. 1864). Naturally in this division we find, xxix. "On the exact Form of Waves near the Surface of Deep Water," xxx. "On Plane Water-lines in two Dimensions," xxxi. "Elementary Demonstrations of Principles relating to Stream-lines;" but we do not meet with "On Stream-lines" (Phil. Mag. 1865), nor "On the Mathematical Theory of Stream-lines" (Phil. Trans. 1871). We have not ventured to take up space with any detailed analysis of Rankine's work, as this has been already admirably done by Clerk-Maxwell ('Nature,' vol. xvii. p. 257), Prof. Tait, and Prof. Osborne Reynolds. The editor informs us that his aim has been to print "such papers as are most characteristic of their author in his capacity of a scientific and mathematical inquirer." He truly remarks that our author was not a popular writer; but "all his writings are marked by a power of statement so clear and logical, that the reader, even should he fail entirely to follow the demonstrations, cannot but be benefited in the attempt to master them." Rankine takes a place amongst the three "mighties" who may be said to have been the creators of the science of thermodynamics. "Thomson had the start; [it] was neck and neck between Rankine and Clausius. But, from the practical point of view, Rankine was alone. And in this respect these papers, as indeed all his others, have a value both intrinsic and as examples of method which even transcends their philosophical value" (Prof. O. Reynolds, 'Nature,' No. 595). Prof. Tait's memoir, though somewhat brief, gives us a very good presentment of the man and the writer, and further tells the story without giving utterance to the "*amari aliquid*" which sometimes mars his writing. The work is well printed; and we have detected typographical errors at very rare intervals.

[When we drew up the preceding Notice, we omitted to consult the "List of Professor Macquorn Rankine's Papers" issued (Nov. 1878) by his friend the late J. R. Napier. From this list we find that 42 communications were made to the Philosophical Magazine. We learn also the reasons for the delay in the appearance of the "Miscellaneous Scientific Papers." Mr. Lewis D. B. Gordon, the originally proposed chief editor, died "more than two and a half

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years" before the above-cited date; and Mr. Napier's illness and subsequent death induced a still further delay. This statement is due to Mr. Millar, who has been looked upon as responsible for the delay referred to in our opening remarks.]

LXXII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 152.]

February 23, 1881.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. A Letter from Dr. John Kirk on an Earthquake shock in the island of Zanzibar.

2. "The Permian, Triassic, and Liassic Rocks of the Carlisle Basin." By T. V. Holmes, Esq., F.G.S.

The district discussed in the author's paper was worked over by him when engaged on the Geological Survey, and consists of those parts of Cumberland and Dumfriesshire which adjoin the Solway. Its southern boundary is, approximately, a line ranging from Maryport to Rose Castle on the river Caldew, and touching the Eden about two miles above Wetheral. On the east and north-east its limits are the immediate neighbourhoods of the junction of the rivers Eden and Irthing, Hethersgill on the Hether Burn, Brackenhill Tower on the Line, and the Border boundary on the rivers Esk and Sark; and in Dumfriesshire the small tract south of a line ranging from the junction of Scots Dyke with the Sark on the north-east, to Cummertrees on the south-west.

The lowest bed in this area is the great Upper Permian or St.-Bees Sandstone, which occupies a belt of country in the neighbourhood of the outer boundary. Directly above St.-Bees Sandstone, in the west of the district, lies a formation consisting of shales with gypsum, which, though 700 feet thick in the neighbourhood of Abbey Town, is nowhere visible, but is known solely from borings—the country west of the Caldew, and of the Eden below the junction of the two streams, being thickly drift-covered and almost sectionless. In the east of the district the St.-Bees Sandstone is overlain directly by a soft, red, false-bedded sandstone, called by the author Kirklinton Sandstone, from the locality in which the rock is best seen, as well as its relations to the under- and overlying beds. But while there is no evidence of any unconformity between the St.-Bees Sandstone and the overlying Gypseous Shales in the west, there is evidence of a decided unconformity between the St.-Bees and Kirklinton Sandstones in the east. In Carwinley Burn (for example), which runs into the Esk at Netherby, only from 200 to 300 feet of St.-Bees stone was seen below the outcrop of the Kirklinton, instead of the 1000 to 1500 feet which probably exist about Brampton on the one hand and in Dumfriesshire on the other. Yet Carwinley Burn affords an almost continuous series of sections, from the (non-faulted) Permian-Carboniferous junction to

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some distance above the outcrop of the Kirklinton Sandstone. As, in addition, the shales underlying the St.-Bees Sandstone are gypseous, both near Carlisle and at Barrowmouth, close to St. Bees Head, the author classed the (Upper) Gypseous Shales as Permian, and the Kirklinton Sandstone as Bunter. Resting unconformably on the Kirklinton Sandstone, in the district between Carlisle and Kirklinton, are the Marls seen on the Eden between Stanwix and Beaumont, and on the Line between Westlinton and Cliff Bridge, Kirklinton. Their unconformity is shown by the fact that on the Line they rest on the lower, or red, beds, and between Stanwix and Beaumont on the upper, or white, beds of the Kirklinton Sandstone. The Marls have therefore been classed as Keuper. So far as the evidence goes, they appear to be very thin and to extend but a very small distance south of the Eden. Lastly, the Lias appeared to the author to be unconformable to all the beds below, and to rest partly on the Gypseous Shales, partly on the Kirklinton Sandstone, and partly on the Keuper Marls. Of the existence of Rhætic beds there was no evidence, all fossils hitherto found having been determined by Mr. Etheridge (our President) to be Lower-Lias forms. But the Lias-sections are so small and few in number, and the ground so persistently drift-covered, that only a boring could settle the question.

3. On *Astroconia Granti*, a new Lyssakine Hexactinellid from the Silurian Formation of Canada." By Prof. W. J. Sollas, M.A., F.G.S.

March 9.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Description of Parts of the Skeleton of an Anomodont Reptile (*Platypodosaurus robustus*, Ow.).—Part II. The Pelvis." By Prof. Owen, C.B., F.R.S., F.G.S., &c.

2. "On the Order Theriodontia, with a Description of a new Genus and Species (*Ælurosaurus felinus*, Ow.)." By Prof. Owen, C.B., F.R.S., F.G.S.

3. "Additional Observations on the Superficial Geology of British Columbia and its adjacent regions." By G. M. Dawson, Esq., D.Sc., F.G.S.

This paper is in continuation of two already published in the Society's Journal (vol. xxxi. p. 603, and vol. xxxiv. p. 89). In subsequent examinations of the southern part of the interior of British Columbia the author has been able to find traces of glaciation in a N. to S. direction as far as or even beyond the 49th parallel. Iron Mountain, for instance, 3500 feet above the neighbouring valleys, 5280 feet above the sea, has its summit strongly ice-worn in direction N. 29° W.–S. 29° E. Other remarkable instances are given which can hardly be explained by local glaciers. Boulder-clay is spread over the entire district; terraces are cut in the rearranged

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material of this, bordering the river-valleys, and at greater elevations expanding over the higher parts of the plateau and mountains. At Mount It-ga-chuz they are 5270 feet above the sea. The author considers that the higher terraces can only be explained by a general flooding of the district. Some of the wide trough-like valleys of the plateau contain a silty material which the author regards as a glacial mud.

North of the 54th parallel and west of the Rocky Mountains similar evidence of glaciation is obtained; erratics are found in the Peace and Athabasca basins. The fjords of British Columbia are extremely glaciated, the marls being generally in conformity with the local features; terraces are scarce and at low levels. The Strait of Georgia was filled by a glacier which overrode the S.E. part of Vancouver's Island; evidence is given to show that this ice came from the neighbouring mountainous country. Queen Charlotte's Island shows evidence of local glaciation. Boulder-clays and stratified drifts are found, with occasional arctic shells.

The author considers that the most probable explanation of the phenomena of the whole region is to suppose the former existence of a great glacier mass resembling the inland ice of Greenland, and that the Glacial period was closed by a general submergence, during which the drifts were deposited, and at its close the terraces cut.

March 23.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "The Upper Greensands and Chloritic Marl of the Isle of Wight." By C. Parkinson, Esq., F.G.S.

In this paper the author described the Upper Greensand as exposed at St. Lawrence and along the Undercliff. At the base of the St.-Lawrence cliff there are hard bands of blue chert from which Astaciform Crustacea have been obtained; and quite recently, in a large boulder of the same material lying on the beach, there were found the remains of a Chelonian, referred by Prof. Owen to the family Paludinosa, and named by him *Plastremys lata*. The presence of these freshwater organisms was thought to imply a connexion with the Wealden continent. The chert-bed, 2 feet thick, was regarded by the author as marking the boundary between the Gault and the Greensand. Above it the author described 56 feet of compact red and yellow sands, of which the first 20 feet are unfossiliferous, the upper 32 feet show traces of organic remains; between them there is a fossiliferous zone 4 feet in thickness, containing *Ammonites inflatus*, *A. auritus*, and species of *Panopæa*, *Cucullæa*, *Arca*, and *Trigonia*, and immediately below this a separate band containing an undetermined species of Ammonite. These sands are followed by 38 feet of alternate beds of hard chert and coarse greensands, having at the bottom 6 feet of inferior building-stone, surmounted by 5 feet of freestone. The latter contains *Ammonites rostratus*, and the cherts various fossils, chiefly bivalves. *Clathraria*

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Lyelli also occurs at this level. Above the greensands come 6 feet of chloritic marl:—the upper 3½ feet fossiliferous, with a base of hard phosphatic nodules containing crushed specimens of *Pecten asper*; the lower 2½ feet compact, with darker grains and few fossils. The author compared the sections of this series given by Capt. Ibbetson and Dr. Barrois; his own views closely correspond with those of the latter writer.

2. "On the Flow of an Ice-sheet, and its Connexion with Glacial Phenomena." By Clement Reid, Esq., F.G.S.

The author considers that the Boulder-clays have been formed beneath an ice-sheet, and consequently there must have been formerly a huge mass of ice, which would have to flow 500 miles on a nearly level surface, and then to ascend a gentle slope for nearly another 100 miles. He does not think a great piling up of the ice at the north pole can be assumed to account for this motion. This he explains by the gradual passage of the earth's heat through the mass of ice raising the temperature of the whole instead of liquefying the surface-layer. As the heat passes upwards it raises the temperature of a particular layer, causes it to expand, and so to put a strain upon the layer above, and then to rupture it. The broken part spreads out, reunites by regelation, and then, receiving the heat from the layer below, again expands, and ruptures the layer next above. Thus the movement is from the base upwards, rather than from the surface downwards.

The author estimates that the ice-sheet in Norfolk was only about 400 feet thick, because Boulder-clay does not appear above that level, but only coarse Boulder-gravel; in North Yorkshire it extends up to about 900 feet. The author considers that the shell-beds of Moel Tryfaen were not deposited under water, but thrust up hill by this advancing ice-sheet.

3. "Soil-cap Motion." By R. W. Coppinger, Esq.

The author described numerous cases in Patagonia where the stumps &c. of trees are to be seen in the marginal waters of the sea and of lakes. These, together with stones and rocks, sometimes simulating perched blocks, he considers to have been brought down by the motion of the soil-cap—a thick spongy mass resting upon rock often worn smooth by the action of ice, and so sliding down the more easily under the influence of vegetation. The appearances are not unlike those due to subsidence; but he points out that all the evidence is in favour of recent upheaval, instead of subsidence.

LXXIII. *Intelligence and Miscellaneous Articles.*

ON THE INTERNAL DISCHARGES OF ELECTRICAL CONDENSERS. BY E. VILLARI*.

WHEN a strongly charged battery is discharged, a characteristic rumbling noise is produced in its interior; the glass of the jars becomes brightly luminous at the edges of the coatings; and

* *Atti dell' Acc. della Soc. di Bologna*, 11 Nov. 1880, ser. 4, vol. ii. 1881.

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heat is evolved, as I have ascertained by introducing one of the jars into an air-thermometer suitably arranged.

Therefore, besides the *ordinary* external discharge of the jar, there is another in its interior, which I will call *internal*, to distinguish it from the former. It takes place along the sides of the condenser where they are destitute of coating; and it is appreciable by the light and heat which accompany it. On measuring the internal discharge by the thermometric dilatations which it produces, we arrive at the following conclusions:—

1. The heat evolved by the internal discharge can be neglected with feeble discharges; beyond certain limits, however, it manifests itself, and increases very rapidly with the discharges themselves; thus a principal means for augmenting this internal heat is to employ jars charged to a very high potential.

2. The internal discharge increases sensibly if the external spark is produced betwen two balls from 10 to 30 millim. in diameter; it is diminished, on the contrary, by almost the half, if the spark is called forth between a point and one of the balls. It is the reverse for the heat produced by the external exciting spark.

3. The internal discharge increases, for a given charge, if the internal coating of the jar be diminished; it diminishes if we augment the coating until it meets the outer coating. From this point it remains almost independent of the extent of the coating within the limits in which I have operated. The reason of these phenomena is complex: they depend in part on the variation undergone by the potential of the discharge with the extension of the coating, and partly on the influence exercised by the different extents of the two coatings upon the number and size of the sparks.

4. The internal discharge is the same with an ordinary as with a spark-discharging jar.

5. The internal discharge falls to zero when the resistance of the external circuit is much increased.

6. The internal discharge appears, *cæteris paribus*, a little stronger with an internal coating of mercury. With this exception, the jar behaves like an ordinary phial with a coating of tinfoil.

The foregoing conclusions, deduced from the thermometric dilatations, are completely confirmed by the luminous phenomena exhibited in the jars, since the brightness and size of the internal sparks constantly correspond almost exactly with the extent of the thermometric dilatations.

7. The internal discharges depend, in my opinion, on this—that each coating induces or excites in the insulating slip a zone charged with opposite electricity to its own, the zones induced by the two coatings being separated by another zone of glass in the natural state. At the moment of the discharge a part of the electricity of the coating and of the electrified zone neutralize each other with production of sparks and heat; hence the internal discharge.

8. The existence of these electrified zones can be demonstrated by the electric figures obtained on projecting upon a Franklin's square of varnished glass, or, better, of ebonite, or on a charged

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Leyden jar, the well-known mixture of sulphur and minium. After the discharge of the condensers those figures can no longer be distinguished; for the electrified zones more or less completely destroy one another at the very instant.

9. When the Franklin's square has its coatings unequal, the neutral zone on the side of the smaller coating, as well as the electrified zone, increases in extent at the moment of the discharge, at least in certain cases.

10. In studying by this method some squares of glass with unequal coatings, or, better of ebonite, with coatings unequal or equal, I have observed that after they had been discharged as usual, the coatings were found to be charged with the opposite electricity to that which they had originally.

Perhaps this method of investigation, modified and extended, may in the future offer us useful indications respecting the inversion of discharges, the influence of the insulators and varnishes made use of in condensers, and the different ways in which the two electricities spread over the insulators—questions to all of which I hope, some day, to return.—*Comptes Rendus de l'Académie des Sciences*, April 4, 1881, t. xcii. pp. 872–874.

THE SECULAR INEQUALITIES IN TERRESTRIAL CLIMATES DEPENDING ON THE PERIHELION LONGITUDE AND ECCENTRICITY OF THE EARTH'S ORBIT.

A paper on this subject, by the Rev. Dr. Haughton, of Trinity College, Dublin, was read before the Royal Society on February 24 last. Dr. Haughton shows that the two inequalities in question depend upon terrestrial radiation only, and in no way upon sun-heat.

Having noticed that the hottest and coldest time of day *follows* noon and midnight by an interval often considerable, and in like manner that the hottest and coldest days in the year *follow* midsummer and midwinter* by an interval often of many days, Dr. Haughton saw in these facts a close analogy with the diurnal tides, which follow the sun or moon's meridian passage by an interval of some hours.

Dr. Haughton was thus led to solve the differential equation on which the problem depends, by assuming an expression similar to those so well known and so long employed in the mathematical discussion of the tides of the ocean.

The result fully justified the assumption of expressions similar to diurnal tidal expressions; for when the differential equation is integrated for a day and summed for a year, all the periodic terms disappear, and nothing is left but terms depending on the perihelion longitude and eccentricity, which represent the exact mathematical expression of the two inequalities first noticed by Adhémar and Croll.

* In the British Islands January 15 is reckoned the time of maximum cold, which is twenty-four days after midwinter.

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The final result takes the form—
Mean annual temperature

$$= k\left((\Theta_0 + a) \pm (\alpha_1 \cos \varpi + \beta_1 \sin \varpi)e\right), \quad \ldots\ldots\ldots \quad (1)$$

where

k = constant,
Θ_0 = mean annual temperature of place,
a = "control"* temperature of atmosphere at place.

α_1 and β_1 are defined by the following equations:—

$2\sqrt{\alpha_1^2 + \beta_1^2}$ = range of annual temperature;

$\frac{\beta_1}{\alpha_1}$ = { tangent of the arc which represents the retardation of the maximum and minimum temperature;

ϖ = longitude of earth's perihelion;
e = eccentricity of earth's orbit.

Using Ferrel's temperature-tables, Dr. Haughton finds the following maximum secular ranges of mean annual temperature:—

Maximum Secular Range.

Latitude.	Northern hemisphere.	Southern hemisphere.
0°	0°·185 F.	0°·185 F.
10	0·375 „	0·585 „
20	1·100 „	0·875 „
30	2·065 „	1·110 „
40	2·750 „	0·985 „
50	3·685 „	0·710 „
60	4·610 „	0·540 „
70	4·985 „	
80	4·925 „	

This table shows that the average maximum effect of the astronomical causes involved in perihelion longitude and eccentricity never can exceed 5° F. in the northern hemisphere, and barely exceeds 1° F. in the southern. At particular localities, where there is a great range of annual temperature, the effect may be somewhat greater. For example, at North Grinnell Land the range becomes 6°·5 F. It will be seen how little benefit this would confer upon that locality, when it is remembered that the present mean annual temperature of North Grinnell Land is 2°·42 F. *below* zero, and that by the secular range it could be raised to 0°·21 F. *above* zero, or depressed to 6°·29 *below* zero.

At Discovery Harbour, Tertiary plant-beds were found by the Arctic explorers, which indicate a *July temperature greater than* 63°·7 F.; the present July temperature of Discovery Harbour is 37°·2 F. above zero, or *only five degrees above the freezing-point of water*. How is this remarkable change in climate to be accounted for? Geologists cannot much longer evade answering such questions as these.

* By this is meant the temperature of the upper layers of the atmosphere of place, which controls the radiation; this temperature varies with the latitude, and is probably always below zero Fahrenheit.

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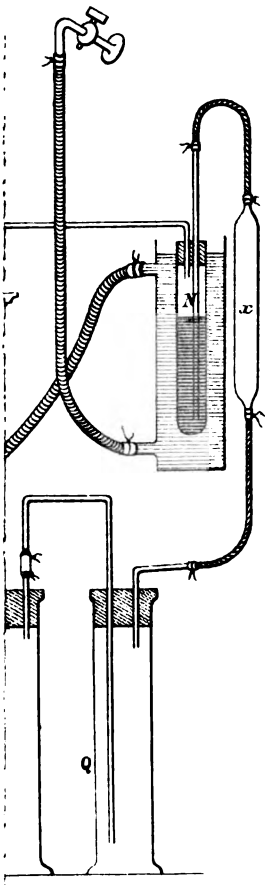

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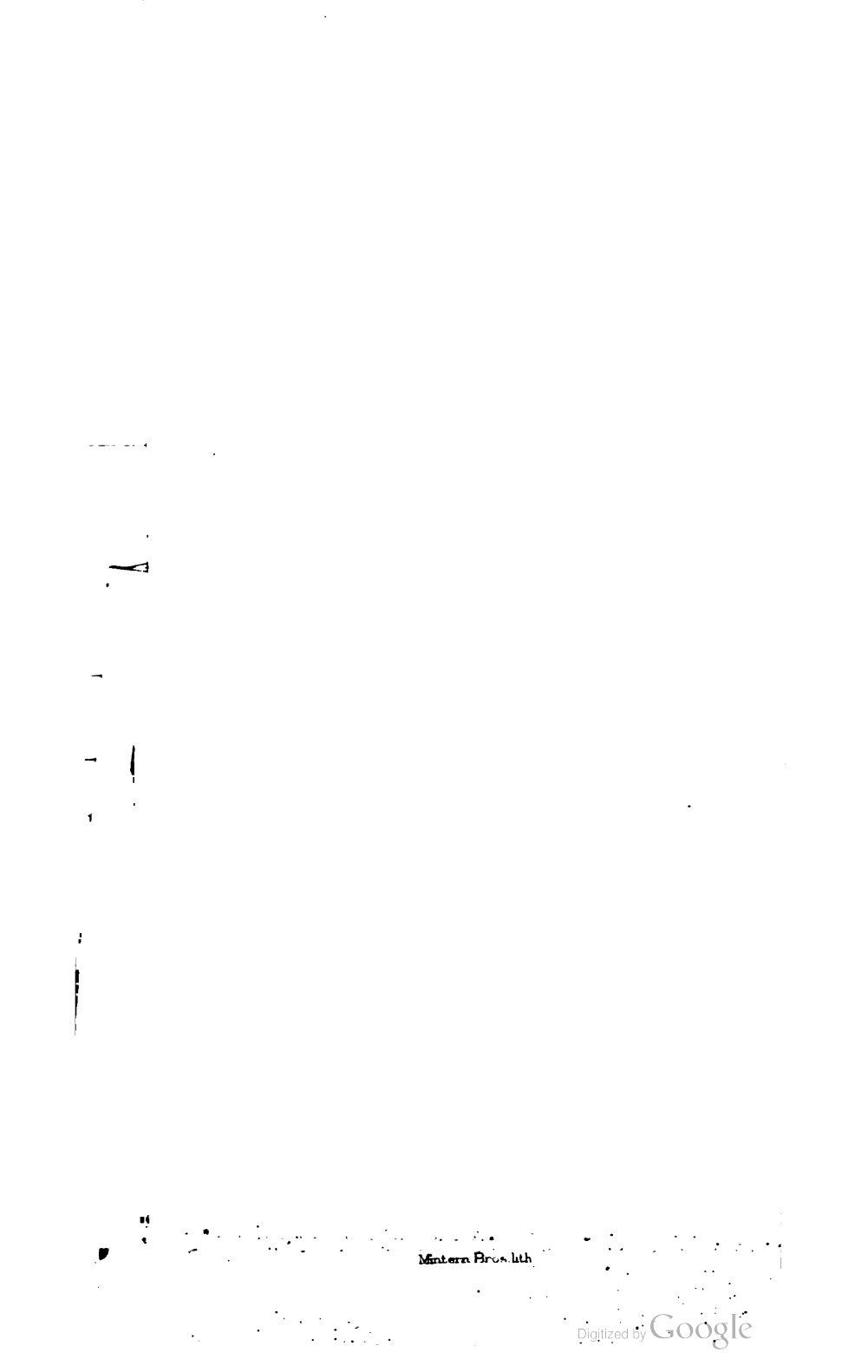

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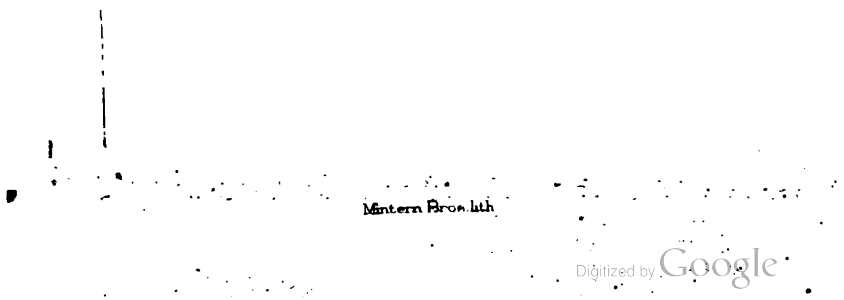

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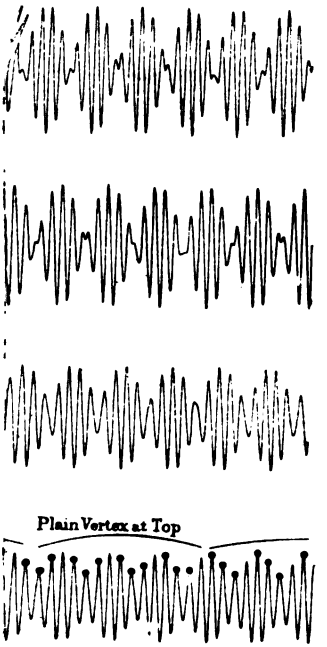

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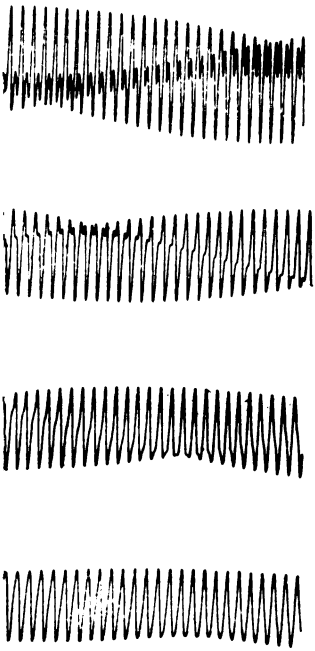

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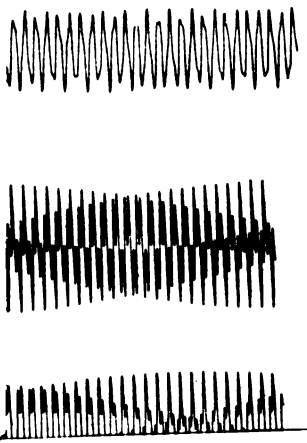

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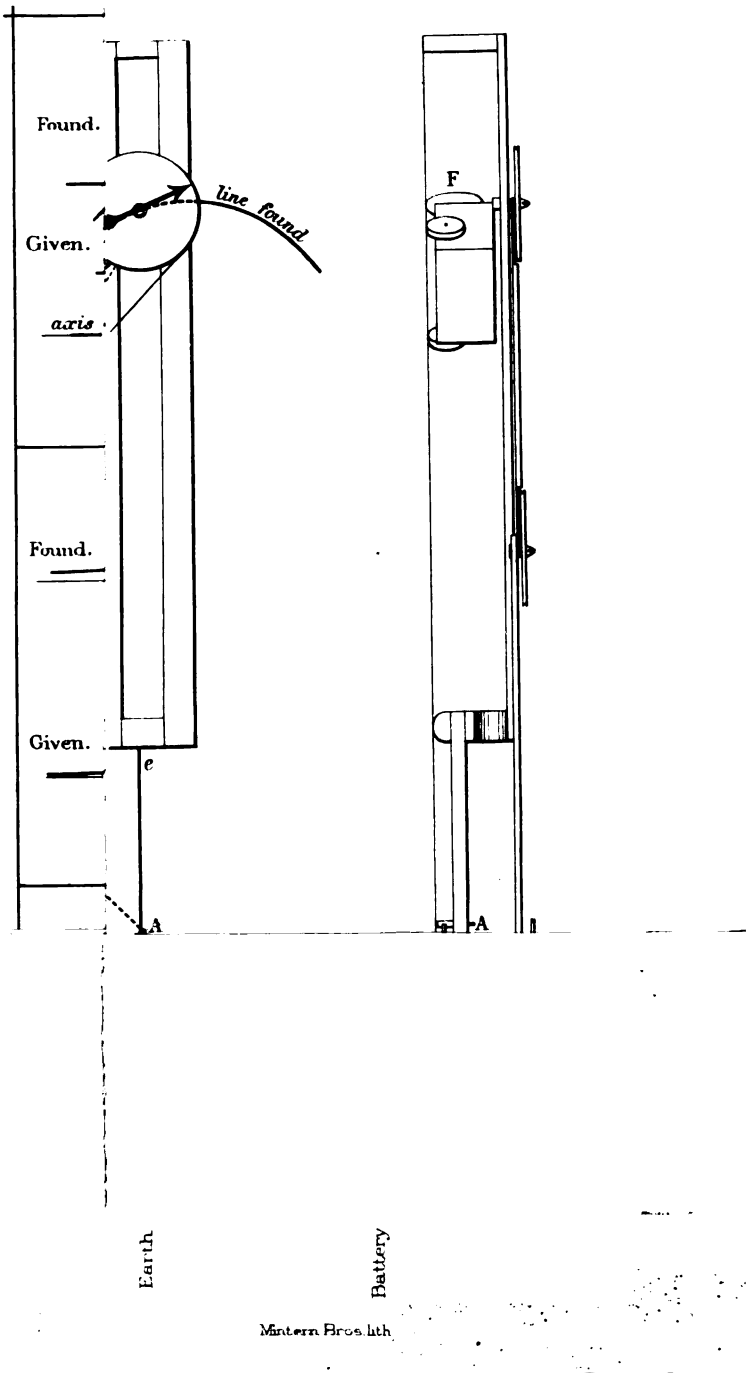

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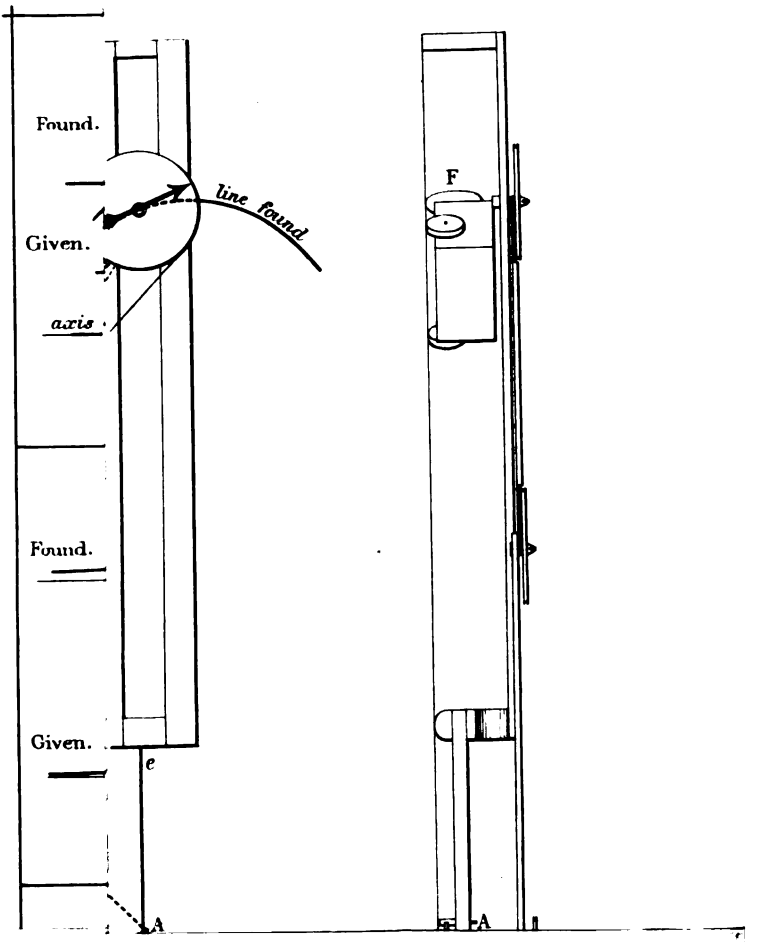

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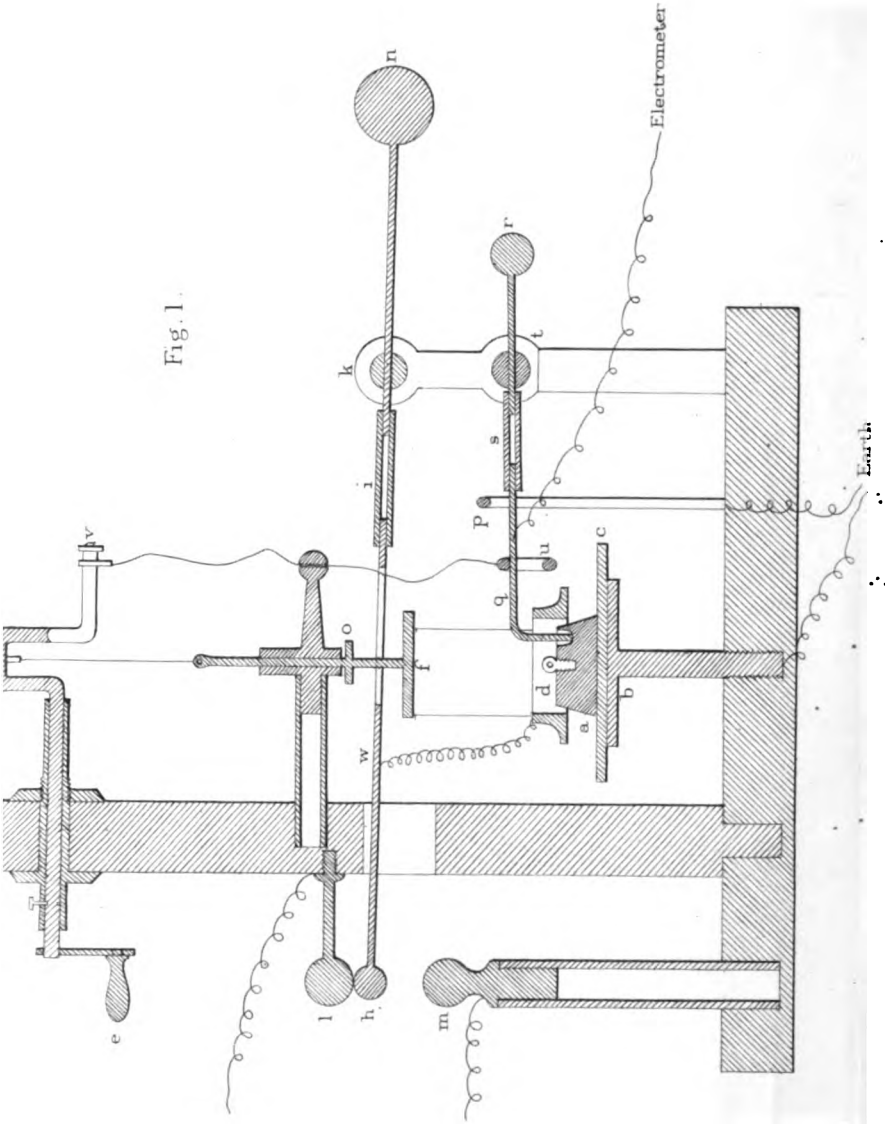

Fig. 1.

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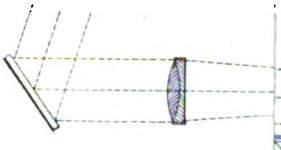
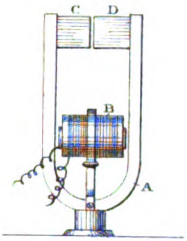


Fig. 11.



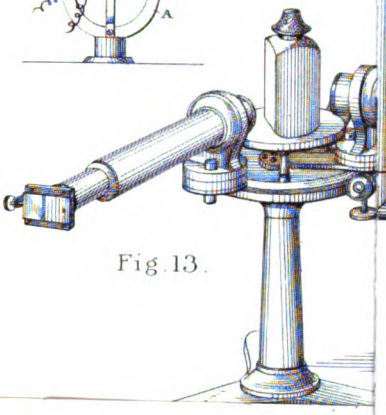


Fig. 13.

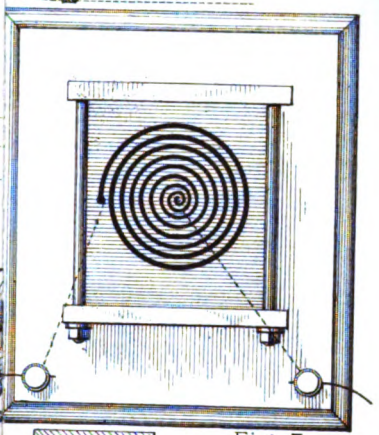
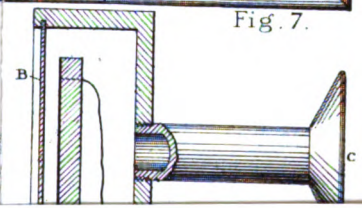


Fig. 7.



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